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RETROSPECTIVE AND PROSPECTIVE VIEWS OF EARLY PERSONALITY DEVELOPMENT

RICHARD Q. BELL

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The term retrospection has been used to refer to the estimation of early phases of development from information available in later phases; the term prospection, to the study of such earlier phases while actually in process (13, p. 15). The thesis of this paper is that a prospective study involves differences in basic approach to the subject matter as well as in methodology, differences not readily appreciated by investigators accustomed to thinking in retrospective terms. The importance of raising such issues for consideration is strongly implied by recent criticism of retrospection (1), current emphasis on the value of direct observation of early parent-child relations (4, p. 563; 11, p. 600), and advocacy of longitudinal research by psychoanalysts (7) who represent a discipline historically associated with retrospection.

To make possible economy of exposition, a hypothetical example of a developmental sequence will be used as a framework for discussion. The following major assumptions commonly expressed or implied by research workers and clinicians have been used to provide a formally plausible example: (a) that family climates and personalities are changing rather than static phenomena; (b) that both personality factors in the parents and interaction effects of these personalities have an impact on child adjustment; (c) that congenital personality characteristics of infants contribute to the direction which a parent-child relationship takes; (d) that personality characteristics of parents have an effect on accessibility of the family to therapeutic and research contacts; and (e) that parental reaction to the quality of adjustment of a child is an integral part of the parent-child relationship.

The problem chosen for the hypothetical example is that of studying a deviant personality syndrome since this has been the most frequent application of retrospection and the purpose of the paper is best served by considering what a prospective study would be like if carried out on the customary subject matter of retrospection. (An example of such a study could not be located.) The customary concern of prospection is with dimensional studies of topics such as intelligence or physical growth within a population selected for "normalcy" or general representativeness rather than for potential to generate deviant personality syndromes. The more

customary type of prospective research is usually described as longitudinal, a term which will not be used in this paper since in current usage it is contrasted with the term cross-sectional but does not clearly imply forward or backward time orientation.

In the example, interpersonal variables are given fictitious names or names borrowed from other fields of science to avoid reification in the mind of the reader and to place the example above the level of current contentions over specific factors of importance in personality development. Variables are assumed to be either present or absent to simplify presentation. Variations in degree could be added if desirable without changing the basic structure of the example. A joint frequency probability theory of the interaction of variables is used. The reader may introduce more complicated interaction results or the names of specific phenomena he considers important if interested in visualizing the implications of his own choice of variables and mode of interaction for such studies.

HYPOTHETICAL EXAMPLE

Unknown to present-day investigators, there is a family climate consisting of two major constellations. One of these, the "I" factor, involution, has a direct effect on the personality of a child, causing the personality to "turn in upon itself" rather than develop adequate relationships with family members and peers. The other is the "E" factor, encapsulation, the more indirect effect of which is to insulate the family as a social system from external influences. Many attributes enter into each constellation. The climate and its constellations result from the interaction of a mother, father, or parent substitute with a child. The mother and father have individual potentials for creating this climate, potentials existing in the form of precursor I and E personality structures. The climate, however, is an emergent phenomenon not reducible to these earlier precursors in the parents.

This over-all climate, referred to as IE, has the effect of inducing a personality pattern referred to as "alpha" in young children with low constitutional viability, provided there is continual exposure from birth to age five or six. The alpha pattern does not occur with high frequency but is of considerable theoretical interest because of the affective and cognitive phenomena it reveals in clear and dramatic form. It is presumed to represent ways in which similar but less easily detected personality patterns are produced in more typical adjustments. The existence of either I or E alone in a family climate is not sufficient to produce alpha. Also unknown is the fact that there are parents whose contribution to the climate is an ameliorative one, acting to dissipate the effect of the I factor in the other parent. The I effect is neutralized in some mothers whose infants elicit special reactions. There are some parents whose effect is neither reinforcing nor ameliorative. In these cases the family climate is primarily the contribution of the other parent. The E factor operates against cooperation of a

family in any study requiring intensive family observation and acts as a source of resistance to family acceptance of help from community facilities when the personality pattern engenders problems for the family. As the child develops, the family climate is gradually modified in order to adjust to the increasingly disturbing effect of the child's reactions. This "B" factor, a buffering reaction, becomes very pronounced in the family climate by the time the child is six, but is reactive rather than a factor contributing essentially to the stream of development. The mother usually shows the B reaction in clearest form.

Current clinical work has resulted in identifying some attributes of both B and I in family situations in which alpha has been found, but it is not understood that the buffering components in the picture should be separated conceptually from the other elements and that the early and late forms of I are different in appearance. There is continuity between the early and late forms of I just as there is between a caterpillar and a butterfly, for example, but, without knowledge of the process of metamorphosis, the early form cannot be identified readily from the late form. It is also not known that the E factor and low viability are necessary to the development of alpha. The entity of alpha is well known, being identified at the time of entrance of the child into his first major social stress with his peer group and adult figures outside the home.

Operating in the same community but at different times are two investigators, one who is interested in studying the alpha process prospectively, and the other who is studying alpha retrospectively. Both believe that certain attributes of the I constellation are somehow related to the development of alpha. These elements have been described clinically largely on the basis of contacts with mothers.

In order to approach the problem in its clearest form and at the earliest point in time, the prospective investigator initiates his study by attempting to select at the time of first pregnancy a group of mothers whose personality pictures resemble the current clinical description of mothers in families in which alpha has developed. Since they are relatively infrequent in a normal, unselected population, he screens a large number of mothers to locate his group. Of necessity, such screening cannot be intensive. In the early phases of screening it becomes apparent that he is unable to locate any mothers who resemble this picture. He could not know this is because the clinical picture contains reactive and emergent elements and only a small number of clues to precursor forms of the family climate he is seeking to isolate. Finally, it becomes necessary to merely separate out for follow-up mothers who show any single trait or characteristic either resembling the clinical picture or presumed to be a precursor. Unknown to the investigator, this screening has yielded a composite group actually consisting of three major subgroups. One group consists of mothers who show characteristics superficially similar to elements in the B complex due to their

reactions to pregnancy, a reaction which is not related to the phenomenon of alpha. Another group is composed of mothers who only have potentials for contributing I to the family climate. A third group consists of mothers having the precursor forms of both I and E. He also selects a control group of mothers not showing any of the characteristics on which the above selection was based.

The prospective investigator cannot anticipate the kinds of infants which will be born to the mothers selected, even if he were alert to this source of variability. Babies born to the mothers in the study may be grossly divided into two groups: a very hardy, viable-reactive group having a special capacity to neutralize the elements in the I constellation contributed by the mother, and a group not having such viability. As with most prospective and retrospective studies, little information is obtained on the fathers except through the mothers. Unidentified by the investigator are five major groups of fathers, when their potentials are considered relative to the problem of alpha development. These groups of fathers show potentials for contributing one of the following to the family climate (a) I and E (b) I alone (c) E alone (d) amelioration (e) nondeterminacy. From the above considerations we can evolve a two by four by five cube of kinds of infants, mothers, and fathers, as shown in Figure 1.

Since our interest is in the development of alpha, we are primarily interested in six cells of the cube in which case density should be highest. In addition to having other distinguishing characteristics, these cells refer to families having nonviable infants. Listing the mother's potential contribution to the climate first, and the father's second, but omitting symbols to stand for buffering and viability, we have the following: On the first row, IE-IE, IE-I, IE-E, IE-N; and on the second row, I-IE and I-E. Only cells I-IE and I-E are sampled adequately in the study since case drop-outs are very high in the families where encapsulation is prominent in both parents or in the mother, who is the main source of contact. This drop-out rate is accentuated by the fact that the existence of nonviable infants with early signs of alpha intensifies encapsulation tendencies in the parents. At the end of the five-year period the study is, in effect, only following up families from two of the cells having nonviable infants in which there is any high probability of alpha development, namely I-IE and I-E. Thus, the study has its heaviest losses in the families in which alpha is most likely to emerge terminally. The investigator, lacking knowledge that the case loss is concentrated in certain subgroups, only sees the loss as a proportion of the total sample. It amounts to roughly one out of ten families in the experimental group over a five-year period. This does not seem excessive in the light of losses in other prospective studies. The nature of the case loss might have been informative had the investigator had more complete personality descriptions of those involved, a condition which is to a large extent precluded both by the encapsulation factor and the requirement of a large sample. The case loss could conceivably have been offset partially

by intensive efforts to maintain cooperation if the unknown E factor could have been anticipated. Research proceeds in the face of the unknown, however, rather than the known.

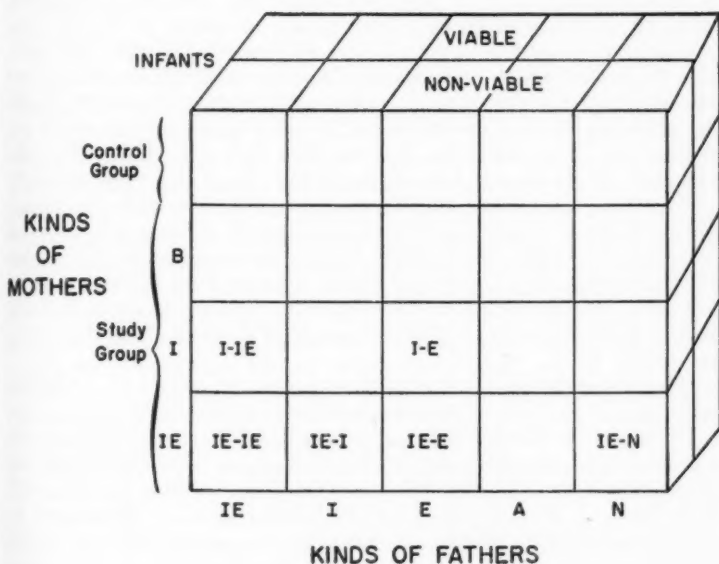


Figure 1

The prospective investigator has followed a large number of families in which elements of the I factor seemed to be prominent, at least initially, when judged by working with the mother who is the main source of contact. It is accordingly reported that the factor has been vastly overrated by retrospective observers, there being many families in the study with children not showing alpha and only a very small number who do show the pattern. The prospective investigator reports a different impression than that normally accorded I since he has seen changes in this factor over time. He emphasizes, on the basis of his early observations, the possible role of the infant's own personality. He is unaware of the role of encapsulation since, as is usual in retrospective studies as well, there is little information on the nature of families failing to cooperate with the study. A tremendous variety of results seemed to emerge, each variety associated with such a small number of cases that it could only be guessed that there were "many complications." Even without the control group, there are 24 different family-child situations represented in the cells not showing high case density.

At the time the children in the prospective study reach the age of six, the retrospectively oriented investigator initiates his studies independently in the same large community area. He works with all cases of alpha emerging in the treatment facilities with which he is associated. Since the mother, who is the primary source of contact, is not the contributor of encapsulation in the I-IE and I-E families, the latter cells comprise the bulk of his case population. The IE-E and IE-IE families are not accessible to study since there is such an encapsulative climate the family has not even accepted referrals urged on them by physicians, school authorities, or other community workers. The IE-I and IE-N cells make negligible contributions to his case population. He is confronted primarily with mothers showing buffering in very prominent form as well as I. Seeing alpha develop in several I-IE families and lacking anything but mediated material on the father, he overlooks the role of encapsulation. He reports his data as indicating a strong association between traits in the I complex and alpha. He has very little information on precursor forms of I, hence tends to merely assume that it has always existed in essentially the later form he viewed.

The conclusions of the retrospective investigator are almost completely at variance with those of the prospective investigator. Both are in error but both have uncovered information which if integrated would provide a more complete cognitive map of the problem area. We might expect the prospective observer to describe I as a factor contributing to alpha only in the presence of other factors. On the basis of his early observations, he would suggest that the infant's own personality is a possible additional factor. The retrospective observer might mention that his cases of alpha seem atypical in physical habitus and temperament but might be skeptical about the role of congenital factors in the infant, feeling that such differences as he saw could be explained as an effect of family climate on feeding and other factors affecting physical growth and reactivity. The prospective observer might discount some elements in the B constellation, noting that they were not present in the early years of contact in the few families in which alpha emerged in his study. He has thus some clues to the nature of the buffering reaction. The retrospective observer would probably express the feeling that only intensive case studies of the type which he has carried out could detect B. He also might feel that the differing description of B offered by the other investigator is again a function of inadequate information due to superficial contact.

COMPARISON OF APPROACHES

Salience

With the formal structure of the hypothetical example as a basis, it is possible to advance certain general propositions concerning the differing

views of the developmental process afforded by the two research approaches. A primary advantage inherent in retrospective studies is salience. Energy is concentrated on cases showing the phenomenon in question. The prospective approach invests most of its energy in cases not showing the phenomenon. Excluding the control group and drop-outs from consideration, 26 quite different interactions of biological, social, and psychological variables were followed up by one investigator in the example, only two by the other. The retrospective observer has capitalized on the power of convergence of influences in the developmental picture. It is as though we have a branching process developing over time. In one type of research a large number of branches are followed up regardless of their eventual direction; in the other type, observation is reserved for the few that terminate in a certain fashion. The prospective approach would not have to follow up all these branches if knowledge were available, making it possible to say that only certain ones would lead to alpha. Such knowledge is not available, though; the phenomenon can only be imperfectly predicted if at all. The investigator of such problems must face the fact he could only select cases so as to achieve salience in later years comparable to that in retrospective studies if a developmental process were sufficiently understood to make high levels of prediction possible. This, of course, is the type of problem in which few investigators are interested.

For a prospective study to achieve even marginal degrees of salience, either a large sample must be followed in order to guarantee the emergence of appropriate cases merely on a chance basis, or some degree of prediction must be achieved with a smaller sample. The low incidence of many personality patterns makes prediction especially difficult. Investigators wishing to study the development of schizophrenia must cope with the fact that adult cases look numerous when one is working in an overburdened treatment center but are a very small part of the basic community population from which they originated. As was obvious in the example, failure to select cases initially which will maximize case density in later years may leave an investigator with a group of subjects either not containing pure and striking examples of the phenomena he is interested in or containing them in such negligible numbers that he has inconclusive data on critical variables. A small number of cases may be extremely valuable in an intensive clinical study in a treatment center. They are much less useful in the less probing and more superficial data collection situation adjusted to the limited motivations of individuals participating in a prospective study. It would appear that the study initiated "just to see what is going on in a few families" may well be the same one reported later in some fashion such as "we saw a few trends but did not have enough of the right kind of cases to prove anything."

At first glance it might seem desirable to use the same sample for a prospective study of several different phenomena rather than limit the

study to one syndrome as in the study of alpha. This is only feasible where the selection criteria for maximizing case density for one syndrome do not minimize case density for others. Furthermore, application of multiple criteria to the same subjects, with contact time per case constant, must inevitably reduce the amount of time devoted to each individual selection criterion. Diffusion of interests and objectives in a study may actually detract from salience by reducing the efficiency of initial selection. One gain might somewhat offset these disadvantages: cases followed up but not useful for the study of one syndrome might be salient for another syndrome. Salience will be a less difficult problem for a prospective study of high frequency phenomena or variables showing adequate range in an unselected population. Dependency and need achievement are examples of less difficult areas to study prospectively.

Returning to the alternative approach for comparison, we see that if the retrospective observer limits himself to case reporting of the personality pattern in question, his observations will lack basic information on factors not leading to alpha. Unless he provides himself with a control series of cases, he is denying himself the opportunity of seeing patterns similar in certain ways to those he has seen but not leading to alpha. There are eight cells other than I-IE and I-E in which the mother shows the I factor alone but in which alpha is not likely to emerge. The latter type of information can offer rigorous and severe reality testing for the host of hypotheses and quasi-hypotheses currently extant in the field of psychopathology.

Magnification

The retrospective approach magnifies the later phases of the developmental process in order to make its extrapolations to earlier phases. This is true even of developmentally oriented psychoanalytic investigations laying heavy emphasis on the first few years of life, since such investigations must proceed through extensively explored response systems as they exist at the time of the study. The "backwards unfolded" case history of an individual who has arrived at the developmental point of interest warrants such elaborate attention, perhaps much more than an initial case of dubious futurity in a prospective study. The case being studied retrospectively has certain intrinsic value since it has persisted through a variety of social, biological, and psychological processes.

The prospective study ideally uses the conceptual telescope in reverse, early phases being scrutinized intensely because of concern for their contribution to the direction of the entire study, later phases being thought of more grossly. Detailed study of the relation between maternal reactions to pregnancy and later reaction of the mother to the newborn such as that carried out by Zemlick and Watson (16) may be crucial in anticipating the kind of relationships being followed up. A woman expecting her first child is obviously in no position to show directly whether she is going to be

indifferent, inconsistent, or guiltily overprotective of her unborn child. Will it be most productive to study her in terms of her differing capacities to relate to dependent and subordinate individuals in her current social milieu? Or will she behave quite differently when the dependent, subordinate individual is her own? How will her attitudes toward her own mother and father be translated into a way of behaving toward her baby? Is her potential for a certain relationship likely to depend heavily on the kind of baby she receives, as was implied in the fictional example? How will she react to a potentially jealous, sadistic, or domineering husband? One finds himself asking whether there are "age-specific mothers": mothers who relate well to children in early years but develop pathogenic relationships at later points in time, or vice versa. This point was raised by Tietze (14) in reporting a retrospective study while raising questions about the adequacy of the approach itself.

Flexibility

It is apparent from the example that the prospective investigation was committed for six years to the task of following up its study group. If the data collection procedure were altered at some point in the process, comparability with data collected at earlier periods would be sacrificed. Since so much effort is expended in selection of cases and early work-ups, abandoning a study group is a difficult decision even if it should appear at times that the phenomenon in question has not been brought into focus by the experimental design.

Hypothetically at least, during an equivalent six-year period the retrospective investigation could shift its attention so as to capitalize on promising leads, change from one type of study population to another, and, if sufficiently concerned about the need for controls, obtain appropriate records on families not showing the phenomenon in question.

Generation of Hypotheses

If salience, magnification of later phases, and flexibility can be said to generally characterize retrospection, it is suggested that the approach is ideally suited for the rapid generation and deductive elaboration of hypotheses. In fact, it could be added that considerable empirical shifting and evaluation of hypotheses could be accomplished while remaining within the confines of this approach. The retrospective investigator can compare his case formulations with those developed for other personality patterns by other investigators or study these other patterns himself. It is by just such means that many retrospectively oriented investigators have arrived at the surprising and disconcerting finding that some key concepts they have advocated, such as infantile personality or early emotional deprivation, are also posited by other investigators as being key concepts for very different types of developmental patterns.

When the retrospective approach is seen as having the major function of contributing a large volume of hypotheses to the scientific economy, certain well-known characteristics of this approach no longer appear as drawbacks. Boldness of extrapolation and belief in the developmental significance of currently manifested behavior seem to be part and parcel of this approach. Doubt and concern about logical and clinical hazards involved in retrospection might create a different orientation to the subject matter, detracting from the richness of the hypothesis formation process. This fact need not mean that in this approach there is no responsibility to carry out deductive checks for consistency of theoretical formulations or to use any empirical tests available. It only means that the investigator can make a material contribution to science, operating almost entirely with retrospective data. Many retrospectively generated hypotheses can be disposed of on the basis of retrospective data as such. For example, Levine (9) and Macalpine (10) have provided reviews of the evidence contradicting the theoretical formulation of specific personality patterns associated with specific somatic dysfunctions. Such considerations have altered the subjective confidence in this formulation without resorting to a prospective approach involving a 35-40 year follow-up of a population hopefully including a sufficient number of cases with such dysfunctions.

Self-Critical Thinking

The very freedom of the retrospective orientation which is so productive of hypotheses also encourages considerable magical and omnipotent thinking. There is nothing in the retrospective approach per se, apart from any special aptitudes a particular investigator may show, to enforce the kind of problem orientation required of a prospective investigator who must visualize the entire developmental process as carefully as possible in order to anticipate problems that would prevent the subject matter from even being available for study. The basic orientation of retrospection does not provide inherent controls present in the forward oriented outlook. The only limit on an investigator's assertions is his own self-criticism and problem-oriented attitude since he is in a situation where he controls all the elements himself. He might assert that maternal dependency is an important element in mother-child relationships in schizophrenia, for example, decide whether it does or does not exist in a series of cases which he reports himself, and if ambitious, decide that it can be extrapolated back to an earlier period. This is the essence of postdiction. He may also decide in his own mind whether it exists in some hypothesized series of control cases, if he is problem minded to that extent. He is spared observation of cases in which such dependency patterns did not result in schizophrenia, if he has no actual controls. He may be making an assertion concerning schizophrenia based on a chance concomitance in a small number of cases studied, the

cases themselves being self-selected perhaps on the basis of dependency itself. The elements in the situation do not in themselves force self-examination on the investigator. The possibilities of making erroneous assertions on the basis of a single case or limited series is well illustrated by Freud's generalization to others of his own apparent invulnerability to cocaine addiction (8, pp. 82-83). Also to be considered, however, is the fact that as a general rule he did not accept hypothesis generation as a complete scientific process in itself. Regardless of whether one agrees with the outcome of Freud's attempts to reconcile his instinct or dream theory with various kinds of repetitive behavior (5, ch. 2; 6, pp. 43-46), the reader can only be impressed with the vigor of the attempt to test his formulation for consistency with existing observational data. Boldness of extrapolation was coupled in Freud with an attitude of mind which stressed use of logical and available empirical checks.

The element of prediction involved implicitly in a prospective study of personality patterns lays bare many otherwise overlooked structures of the problem to be solved. For example, the subjective strength of an investigator's belief in a limited group of factors may be modified considerably by mere confrontation with the actuality of trying to select families that will produce a given kind of personality. If he expects processes selected for an ongoing study to have some chance of moving in the direction desired for research purposes, he must develop at the very least a personally satisfying stand on many questions. He should be much more interested perhaps in interpreting results of studies which apparently indicate families may be inconsistent in their effects on children (15). He may have to decide whether possible contributions of the child's constitutional characteristics and the effects of the changing family situation as new children are born need to be taken into consideration in the research plan. The mere consideration of such factors represents a conceptual improvement.

Unchanging Neural Traces and Parents

The magnification and backward extrapolation of later phases of a developmental process carries with it some peculiar hazards. Since Bach (1) has already reviewed the logical and clinical implications of placing complete faith in retrospection based on memories, it seems necessary only to summarize these. It should be noted that these implications are probably not fully appreciated as being implicit in retrospection's use. They would appear to be largely unacceptable at first consideration, even if we might assume logically that they would regain acceptability if considered as alternatives to limits involved in the prospective approach. In general, retrospection used for estimation of earlier developmental processes from memories involves the assumption that memory is a static process requiring

only the reactivation of a neural trace perfectly recorded initially, perfectly stored, and capable of being reactivated intact, with appropriate clinical procedures, relatively uninfluenced by intervening events or by the situation existing at the time of recall. Freud's acceptance and subsequent rejection of the role of actual sexual seduction in hysteria is a familiar clinical example of the problem involved.

Bach cites neurological, social-psychological, and psychoanalytic research work supporting a much more dynamic and currently acceptable theory of memory as a reconstructive process heavily dependent on the interpersonal situation where it occurs. The therapist or interviewer, his value systems, and the social situation of the interview are seen as integral components of the situation in which memories are produced. Most clinicians are well aware that silence, bodily movement, verbal reaction and intonation are all quickly sensed in some way and interpreted, even though at a very low level of awareness, as indices of the interviewer's value systems. Studies of the way patients learn a therapist's value systems (12) point to the need for elaborate cross-checks on material presented by patients in psychotherapy. If the patient is in fact merely "feeding back" many of the therapist's viewpoints, the system approaches a closed circular process in which erroneous views can be perpetuated indefinitely.

Generalization from direct observation of a parent-child relationship at a point in time much later than the period about which there is theoretical interest makes equally unacceptable assumptions, even though the inadequacies of interpersonal recall are bypassed. In this instance, the assumptions concern the fixity of social relations over time. In the example it will be noted that the prospective investigator was viewing precursors of constellations which the retrospective investigator saw in later form. Similarly, from many current case reports in the literature we get the impression that the mothers of adult schizophrenics have always been obsessive, tense, emotionally cold and overprotective, just as they appear in a current observation situation. This view leaves the mother no latitude for change from one role to another as her family situation has evolved over time. If a retrospective investigator asserts that he is making observations on enduring, stable manifestations of personality in parents and hence can extrapolate these backwards in time, he should present data to support the contention that he has detected genotypes rather than phenotypical manifestations which are dependent on a particular stage of the family history. At the present time it is not at all clear that there are readily acceptable bases for making distinctions between such core traits and their more varied manifestations.

In general, the attempt to describe a growth process by an isomorphic projection of present events to the past runs the risk of overlooking complex emergent manifestations, changes of the form in which a phenomenon is manifest.

SUMMARY

The hypothetical example has been designed to illustrate the thesis that neither retrospection nor prospecting can be advanced as having any exclusive claims to validity, both approaches being conditioned by peculiar advantages and disadvantages. A criticism of one approach without discussion of the limitations of the alternative neglects the relativity of data from either. It is more pertinent to consider ways in which the two approaches can best interdigitate in the total scientific effort. One approach can be expected to generate a tremendous number of new leads while having few integral mechanisms to alleviate what at some times appears to be a state of ideological surfeit. Adequately controlled retrospective studies can, however, accomplish a much greater reduction and sharpening of hypotheses than has been customary. Many hypotheses can be disposed of on the basis of retrospective data alone, making time consuming prospective tests unnecessary. Prospective studies provide a still more rigorous means of reduction and sharpening of hypotheses, in addition to providing new leads unique to the type of outlook implicitly involved. Complex metamorphic growth processes can be followed that would be overlooked if an investigator were committed to an attempt to explain past events with extrapolations from present events. Prospective studies run the risk of not providing relevant information after proceeding over long-time spans to a termination, the possibilities for generating salient data being limited by the difficulties of predicting the eventual courses of developmental processes. Prospective studies of normal growth processes are less vulnerable to this problem.

Shorter-term prospective studies seem much more defensible in view of the current pace of cultural change and scientific development. These studies will contribute less to the over-all movement of the field if limited to evaluation of hypotheses originated in another era, many having already been disposed of on the basis of very compelling retrospective data. In keeping with developments coming from both research approaches, it would seem desirable to shift from one short-term prospective study to another, study short segments of a total developmental process and articulate these segments conceptually or with special studies of overlapping segments (2), and make maximum current use of individuals studied many years ago for some earlier purpose. A study by Frazee (3) of children who later became schizophrenic typifies a number of investigations undertaken with the objective of capitalizing on earlier treatment of research records while working on problems with the theoretical orientations of the present.

A further implication is that regardless of the study design selected by an investigator, there are likely to be important conceptual gains realized from merely placing oneself in the frame of reference of another research approach.

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ISSUES RAISED BY ECOLOGICAL AND "CLASSICAL" RESEARCH EFFORTS¹

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The authors have engaged in a number of research studies involving ecological and classical methodologies. These experiences have presented problems, and perhaps understandings, which we would like to report.

As an initial frame of reference, we might consider a simple schema involving a milieu stream on the one hand and a behavior stream on the other. By milieu stream we refer to the flow of environmental conditions within which the individual behaves—his social associates and their behavior, his activity props and arenas—and the changes of these over time. The milieu stream may be visualized as supporting, provoking, or restricting the behavior stream.

Ecological studies in psychology can involve observation of behavior in relatively total milieus. An example of this is *One Boy's Day* by Barker and Wright (1). A study by Gump, Schoggen, and Redl (2) consisted of day-long records of a boy in contrasting milieus: home and camp. This study demonstrated significant differences in milieu streams and concomitant differences in the boy's behavior stream.

A second type of ecological study investigates behavior in selected settings within a milieu. For example, continuing specimen records were taken of the behavior of eight boys in three camp behavior settings: swimming, cook-out, and dining hall. Three different cabin groups were represented. Adult leadership and child associates were similar in all three settings. A thesis by Millen (5) compared the number of aggressive acts for each boy in each setting. See Table 1.

The generality of an ascending frequency of aggressive acts for each boy as he went from swims to cook-outs to dining hall was very clear. Means were 7, 28, 48. The F ratio for between-settings differences was significant at the .01 level; the F ratio for consistent differences between boys did not approach significance. Behavior would seem predictable on the basis of setting entered, somewhat independent of individual differences.

A third type of ecological study involves the description of an *incident* in the milieu stream and concomitant behavior stream changes. Here, as

¹This paper was offered as an address at the 1959 annual meeting of the Society for Research in Child Development at Bethesda, Maryland. The studies referred to in this paper have been supported by grants M-550 and M-1066 from the National Institute of Mental Health, U.S. Public Health Service.

in the preceding types of investigation, neither milieu nor behavior are controlled or altered. One hopes that the milieu will provide a sufficient number of differing types of events and concomitant behaviors to approximate a classical independent-dependent variable study. In one such ecological study of this kind, the problem of interest was the effects of a teacher's control or "discipline" maneuvers upon *audiences*—not targets (4). For example, what effect is produced on Billy when a teacher warns Johnny, "You'd better stop talking and start listening!" In this case, the milieu change is not directed at our subject; objectively, this change is simply information to which he may or may not overtly respond. We were interested here, then, in the "spread" or "ripple" effect of teachers' control efforts. Trained observers were sent into beginning kindergarten classes to watch and record these control techniques and to make specimen records of the behavior of the child physically nearest to the child who was the target of the teacher's control effort. When the resulting 400 incidents were analyzed, it was possible to characterize both the milieu changes (teacher control) and the behavior changes of the audience child.

TABLE I
NUMBER OF AGGRESSIVE ACTS BY THE SAME BOYS
IN THREE CAMP BEHAVIOR SETTINGS

Camper	Swim	Cook-out	Dining Hall
A	0	11	58
B	0	34	54
C	6	18	24
D	10	11	97
E	9	26	28
F	12	78	25
G	7	15	32
H	13	33	64

Analysis identified 40 types of behavior which a teacher might employ in her control effort. For instance, the teacher could explain why the deviant behavior was unacceptable; she could walk toward or touch the target; she might occasionally use an angry tone or pull or push the deviant. These 40 types of control elements were coded into three major categories: some increased the *clarity* of the teacher's message by describing what was wrong, why it was wrong, or what good behavior should be substituted; some increased the *firmness* by adding a stronger "I mean it" quality to the control effort; and some imputed a harshness or *roughness* aspect to the attempt at control. In summary, teachers' control behavior varied along dimensions of clarity, firmness, and roughness.

The reactions of the watching child could also be classified: often there was *no overt reaction*; frequently, though, a child would increase or decrease his *conformance* with what the teacher wanted; occasionally, the watching child would show signs of *behavior disruption*, i.e., he would begin to look anxiously about, to wander aimlessly, or to withdraw into a short daydreaming state.

Among other results, we found that techniques of high clarity resulted in increased conformance ($p < .01$) but had no effect on behavior disruption; rough techniques, on the other hand, had no effect on conformance but did increase behavior disruption ($p < .02$).

In this study the piece of the milieu of interest—teacher control—was preselected. There were no established reasons for thinking that this bit of milieu change would significantly affect behavior; in fact, logic might suggest that a control technique directed at somebody else would have little effect on one's own behavior stream. But, we were fortunate. The milieu shifts lent themselves to meaningful classification, and enough overt behavior was affected to yield "results." Not all preselected milieu pieces or variables would give results, if ecological—as opposed to classical—methods are used. We wish to return to this point later.

SOME EXPERIENCES WITH CLASSICAL METHODS

The most usual situation in psychological research is one in which independent and dependent variables have already been selected. The preselection may be based on theoretical concerns—we are continually told it should be—or upon assessments of ecological validity, i.e., the investigator believes that certain milieu differences are really important and he decides to investigate them under controlled conditions. This controlling effort impinges on our simple schema at two points: it involves wrenching a milieu phenomenon out of its context, and it usually requires some interference with the behavior stream. The implication of these twin distortions involved in controlled research are illustrated by the following two studies.

The investigators decided to test the effects of supportive *vs.* threatening control techniques upon nontarget college students in a controlled experiment. It was found that a punitive—as compared to a supportive—technique directed at a class member resulted in significantly lowered evaluations of the instructor's competence, likability, and fairness. We had other data, however, which affected the interpretation of these results. On a post-incident questionnaire, students who witnessed either technique were surprised that one of their college instructors would take time out to correct a student for coming late to class. What we found out, then, in our effort to produce a clean experiment was how students evaluate the instructor using supportive *vs.* punitive control techniques *when they are surprised or when they see the behavior as atypical*. What would happen in a milieu when such behaviors are *reasonably expected* of instructors (as in a high school) we did not discover. Our clean independent variable,

wrenched out of context, got itself attached to another variable, thus sharply limiting the extent to which results could be generalized.

A second experiment can be cited as relevant to the issue of how artificial constraints on behavior streams can yield results which, although experimentally neat, are of tenuous value for generalization to the significant aspects of the problem under study. A study by Gump and Sutton-Smith (3) investigated the reactions of poorly skilled players when they were put in more or less difficult game positions or roles. For example, the *It* position is more demanding in some games than it is in others.

The games selected were variants of Pom-Pom-Pullaway. In this game, an *It* in the center of a rectangular playing field attempts to tag opponents who run to and from safe areas at each end of the rectangle. In one variant of the game, which gives high power to the *It* position, the child who is *It* calls the turn—he says when runners can attempt to cross from one safe area to the other. In another variant, which gives low power to the *It*, the runners cross whenever *they* choose. In one phase of the experiment, pre-selected slow runners were assigned to high-power *It* positions; in another phase, to low-power *It* positions. The hypotheses were that poorly skilled boys will be more successful in high-power rather than low-power *It* positions, and that scapegoating of these inept boys will be less when they are in high-power rather than in low-power *It* positions. Results amply confirmed these hypotheses: in a low-power as opposed to a high-power game position, low-skill boys experienced more tagging failures and uttered more defeatist and distress comments. They were teased and combined against by their opponents only in the low-power *It* position.

However, we might ask what happens when the milieu is not controlled and when behavior is not restricted. Our observations in gyms, playgrounds, and camps indicate the following: (a) poorly skilled boys do not often get involved in games they cannot manage; (b) if they do get involved, they often manage to avoid difficult roles by not trying to win such a position or by quitting if they cannot avoid it; and (c) if they occupy the role and are having trouble, the game often gets so boring to opponents that these opponents let themselves be caught in order to put the game back on a more zestful level. (In our experimental game, which adults watched, no opponent would let himself be caught.) The experiment did not measure the impact of low-power *It* roles on the unskilled as this situation occurs as a part of children's natural behavior streams. It showed what happens under somewhat desperate circumstances—circumstances created and sustained by interfering adults. The experiment probably does suggest hypotheses as to why certain games and roles are avoided by inept children, and it shows the extent to which children can scapegoat when conditions are artificially favorable.

The moral we have drawn from such experiments as the college and games studies is roughly this: If one wishes to generalize from his experi-

ments to natural milieus and behaviors, considerable ecological knowledge is necessary. Variables from the general milieu may intrude in the cleanest experiment; behaviors responsive to experimentally induced forces may be even more responsive to different forces when the behavior stream can take its natural course.

LIMITATIONS OF THE ECOLOGICAL APPROACH

Although the deficiencies of controlled or experimental methods are troublesome, attempts to use ecological methods for certain types of problems can also involve difficulties. One of these relates to economy of research effort.

After we had investigated the ripple effects of control techniques in kindergarten, we wished to make a more complete study in a milieu which might provide a greater variety of these incidents; we wished also to have a more complete account of the audience child's behavior prior to the incident in order to compare this with his post-incident behavior. A public camp was selected and two subbehavior settings were observed: cabin clean-up and cabin rest period. Complete but short specimen records were taken for the leader's behavior by one observer; similar records of individual campers by another. Over a six-week's period of work, involving six full-time observers, some 46 hours of camper behavior were recorded. The total number of incidents to which our audience child made some overt active response was only 139. A number of things occurred to reduce the number of incidents: leaders often directed control techniques to an entire group so our audience camper was a target and these incidents could not be used; many other leader control efforts were either not heard or ignored; finally, many of those to which our audience child attended, produced no other behavioral stream change. Our final analyzable yield turned out to be about one incident per 18 pages of specimen record material. This low yield was a sobering experience. We think we shall be able to say something about the effects of various control techniques as revealed by this material but certainly not as much as we had hoped when we started. We became impressed with an obvious but little emphasized facet of classical research: in classical research, the milieu conditions of interest are presented with relatively high frequency, insuring a much greater yield of incidents in which the effects of these conditions may be studied.

We can think of some research problems for which our records of camp behavior would be ideal, but for our problem, there was less than a generous amount of data.

Our reflections upon this experience go in several directions. One is that critical incident recording is cheaper if one is determined to study just these incidents and is unconcerned about their ecological frequency and importance. And in terms of economics, direct experimentation would often be even cheaper than critical incidents research.

Another reflection is that the low yield was itself a finding: in essence, our data informed us that the ripple effect of leader control techniques, as judged by *overt* audience reaction, was ecologically infrequent (and perhaps unimportant), at least for these kinds of situations.

The only reservation we would put on this conclusion that ripple effects were unimportant in the above situation deals with another matter of interest. It was mentioned above that the effects of numerous leader control attempts were difficult to assess because our audience child made no overt active response to them. The question of what hidden difference the incident made is still unanswered. At the beginning of our remarks we referred to a simple schema of milieu stream and concurrent behavior stream. Occurrences in *covert* behavior are often unrepresented in the immediately occurring overt behavior stream.

In terms of our interest in ripple effects, our point is this: a particular control incident to which an audience child makes no overt active response could result in covert changes. For example, it could result in changed attitudes about the person emitting that control technique. We interviewed 120 beginning high school students about the most recent control incident that occurred in their class. Various interviewing devices were used to make the recall as vivid and accurate as possible. Then we asked an open-ended question regarding possible changes in their attitude toward the teacher resulting from what they saw. About one student in seven said his attitude toward, or assessment of, the teacher was significantly affected by his one observation of how this teacher interfered with the behavior of another student. We are almost certain that if these students had been under observation at the time of the initial incident, no naturally occurring overt behaviors would have telegraphed this change in attitude toward their teachers. Insofar as interviewing involves an interference with naturally occurring behavior, such interference might seem necessary if covert changes, especially those of an attitudinal nature, are to be investigated.

Relevant to this problem of tapping the covert, we would like to describe a final study. Since we are presently concerned with misconduct and how it is handled, it appeared worth while to investigate children's concepts of misconduct. The procedure was highly direct; we asked 415 first-grade children the following questions: "What's the worst thing a child can do at school?" and "Why is that so bad?" We also asked: "What is the worst thing a child can do at home?" and "Why is that so bad?" Children will answer such questions differently depending upon their sex, the punitiveness of their present teacher, and the relevant milieu, i.e., home or school. Ideas of wrong and its consequences differ pervasively when home *vs.* school, as well as when sex, or punitiveness of teacher are the independent variables. For example, for the total population, there is significantly more concern with physical aggression at home than at school ($p < .001$). And an interesting twist to our data occurs when analyses for

boys and girls are run separately. For some dimensions of misconduct, school has a different degree of impact upon girls than upon boys. For example, school reduces girls' concern with problems of physical aggression significantly more than it does for boys.

For persons interested in the development of children's conscience, such data may be provocative; however it would be much better to learn why these differences occur. Are the sex differences at school a result of differential boy vs. girl treatment by female teachers or are they a reflection of some general milieu factors which push boys rather than girls into aggression-loaded situations? It seems to us that an ecological approach to this sort of problem is imperative. It is necessary to find out what these two milieus are doing in order to understand why different consciences seem to be developed for each. In many cases, a finding from ecological investigation will suggest a more controlled follow-up; in this case, the direction of suggestion goes the other way: from a finding developed by interfering with the behavior stream to an investigation of the milieu.

SUMMARY

For many of us it is probably a truism that both ecological and experimental efforts are required if we are to understand children's behavior. We have attempted to specify some of the issues relevant to that truism. Portions of studies which highlight the coerciveness of milieu factors have been used as illustrations. Study of milieus and of the naturally occurring behavior within these milieus are valuable in pointing up certain ecological validities, i.e., what real milieu conditions and shifts occur with enough frequency or behavior impact to be worthy of study.

Also cited were twin difficulties of premature or arbitrary experimentation: misrepresentation of a milieu factor which may occur when this factor is wrenched from its context, and possible misinterpretation of the meaning of a behavior when the behavior stream has been arbitrarily rechanneled by experimental methods.

However, it also seems that full-scale ecological approaches can be wasteful when the particular milieu factors or independent variables have already been decided upon. This occurs when the observed milieu and the behavior streams present too few recurrent similarities from which to make inferences. Finally, the problem of tapping the covert behavior stream remains an issue. Children seem to be changed by milieu changes without reasonably immediate overt indicators of this change. Interference with the behavior stream, to the extent required by interviewing or other testing, seems essential if some of these covert changes are to be studied. Sometimes this interference may yield issues which point to ecological approaches for their resolution.

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SOME IMPLICATIONS OF CURRENT CHANGES IN SEX ROLE PATTERNS¹

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In recent times it has been the fashion to cry havoc and sound the alarm when discussing alleged changes in sex role patterns. The topic itself has had a certain band wagon effect so that one could scarcely open a Sunday supplement without being faced with scare headlines about women in pants and the dire consequences of contact with dishwater to the male psyche.² All sorts of "experts" have had a heyday with "mannish" women and "emasculated" men, on the basis of very little evidence for the existence of either in unusual proportions. In view of undisputed interest in this topic, it seems desirable to take a sober look at the facts and make a considered assessment of the changes that actually have taken place, or are taking place, on the basis of systematically collected empirical evidence.

When we make such evidence the criterion for our evaluations, we find there is surprisingly little to go on. For one thing, to talk of *change* in sex role patterns implies comparison with some state before the change took place. In this case, it demands that we establish some temporal baseline, i.e., we must answer the question: change since when? An examination of the popular press seems to indicate that much of the recent and current hysteria had its beginnings in conditions at the close of World War II, when some of the women who had been lured out of private life for service in wartime industries and as substitutes for men withdrawn from essential public services refused to be pushed back again. The cries of anguish of

¹This paper is based on an address delivered at the annual meeting of the New York Society of Clinical Psychologists, May 23, 1959. The data reported here were collected as part of a project supported by Research Grant M-959 (C, C-1, C-2, C-3) from the National Institute of Mental Health, Public Health Service. The author wishes to thank Dr. Frank Hardesty, Research Associate on the project, for his skill in collecting a large portion of the data reported here and for his cooperation in handling the analysis.

²See, for example, these articles: Ward Cannel, "Is the American Male Man or Mouse?" *Washington Daily News*, Dec. 11, 1958; Bruno Bettelheim, "Fathers Shouldn't Be Mothers!" *This Week Magazine*, Apr. 20, 1958; Jean Libman Black, "Husbands Shouldn't Do Housework!" *This Week Magazine*, Sept. 8, 1957; Dorothy Barclay, "Trousered Mothers and Dishwashing Dads," *New York Times Magazine*, Apr. 28, 1957; Mike Wallace and John A. Schindler, "Are the Two Sexes Merging?" *New York Post*, Oct. 17, 1957.

the would-be persuaders are preserved in most of the post-1945 issues of the "slick" magazines addressed to homemakers. Our assumption, therefore, is that the change we are assessing must have taken place roughly within the last twenty years, between the beginning of World War II and now. If this is the case, we must look for reliable empirical material, preferably in quantitative form, depicting sex role patterns around 1940 and a set of similar measurements of current patterns, and compare them, before we can really know what, if anything, has happened to sex role patterns within the last two decades.

Anyone who has tried to find sets of validly comparable sex role data gathered over a twenty-year interval will acknowledge that the pickings are rather sparse. We have some statistics for who-does-what in limited samples of current families,³ but with what shall we compare them? This just does not seem to have been a burning question in 1940—certainly not important enough for any systematic records to have been made.

The best evidence we have deals with only a small segment of sex role activities in adulthood and derives from census data. I refer, of course, to the figures depicting the striking rise in the number of women in the labor force during the last twenty years. Before we consider this evidence as a definitive criterion of change, however, I would like to quote a few words of caution by Smuts, who has been intimately involved in the intensive study of human resources conducted by the Conservation of Human Resources Project.⁴ He writes:

The only source of comprehensive data on the number of working women and their occupations is the U.S. Census. Before 1940 the Census counted these persons, aged ten years and over, who were "gainfully occupied." A gainfully occupied person was defined, essentially, as one who pursued with some regularity an activity which produced money income. Since 1940 the Census has counted persons in the "labor force," which is a much more precise approach. The labor force includes all persons aged fourteen and over who work for pay or profit for any length of time during a given week or who seek such work; and those who work for fifteen hours a week or more in a profit-making family enterprise, even if not paid. . . .

Comparison of Census data on women's employment in different years must be undertaken with extreme caution. The definition of a gainfully occupied person was much narrower in 1890 than in later years. The 1890 Census counted a person as gainfully occupied only if he reported an occu-

³One source is an unpublished study by Lois W. Hoffman, Ronald Lippitt, and others, at the Research Center for Group Dynamics, University of Michigan, in which 450 middle-class families in Detroit were surveyed. The study was reported by John Sombower in *The New York Journal*, November 12, 1957, under the headline "Hear Ye, O Husbands, Dusting's Not Thy Destiny. . . ." References to results of this survey can also be found elsewhere (1, 9, 13).

⁴Established at Columbia University in 1950 as a cooperative research undertaking, the Project has led to major publications concerning the uneducated, the Negro potential, the ineffective soldier, and women as workers.

pation "upon which he chiefly depends for support, and in which he would ordinarily be engaged during the larger part of the year." This definition excluded most of the large number of women who earned money through homework and many of those who worked irregularly away from home. In addition, a great many who qualified under the definition were not counted. Because most women did not work regularly for pay, and because the Census instructions emphasized the circumstances under which women should not be counted as gainfully occupied, Census takers were likely to be careless in recording women's work. This tendency was reinforced by the *ad hoc* character of the Census organization and by the tendency of the respondents to reflect the prevailing views of women's proper role by concealing the employment of female members of the family. As a result of all these circumstances, any woman who was both a housekeeper and a paid worker, or a student and a paid worker, was likely to be recorded only as a housekeeper or student.

It is particularly important to keep this in mind when evaluating analyses of long-term trends in women's work. The increasing employment of women shown by the Census data reflects not only the growing employment of women for pay, but also the growing willingness of respondents to report women's work, the broadening of Census definitions, improvement of the Census organization and procedures, and the decline of homework, which could be more easily overlooked than work away from home (20, pp. 157-159).

It seems clear from the above that even Census data are not a reliable indicator of the extent of social changes. Granting the validity of these qualifications, however, it still seems evident that there are more women in the labor force than ever before, and of these a larger proportion are wives and mothers than ever before. This is one definite change in sex role activity for which we have evidence and we might start our search for implications by asking what impact this change has had. First, however, it would be well to again attend to some admonitions from Smuts:

Emphasis on the new work of women, however, should not be allowed to obscure an equally important fact. Today, as always, most of the time and effort of American wives is devoted to their responsibilities within the home and family circle. This is true even of those who are in the labor force. Since 1890 the demands of paid work have become much lighter. The normal work week has decreased from sixty to forty hours; paid holidays and vacations have become universal; and most of the hard, physical labor that work once required has been eliminated. Because of these developments, many women can work outside the home and still have time and energy left for home and family. Moreover, most working mothers do not assume the burdens of a full schedule of paid work. Among employed mothers of preschool children, four out of five worked only part time or less than half the year in 1956. Among those whose children were in school, three out of five followed the same curtailed work schedule. And even among working wives who had no children at home, only a little more than half were year-round, full-time members of the labor force (20, pp. 36-37).

As the last quotation suggests, this change in feminine roles apparently does not mean that women are abandoning their traditional family responsibilities to seek personal glory in the market place. It does not even seem to mean that women are particularly intent on competing with men, contrary to the accusations that have been frequently hurled at them. On this point, Smuts remarks:

Today, as in 1890, the great majority of working women have little interest in achieving success in a career. . . .

In recent years employers have been able to attract millions of additional women into the labor force without changing the relative levels of men's and women's pay, or greatly expanding women's opportunities for advancement. The decade between 1945 and 1955 was one of booming prosperity, labor shortages, unprecedented peacetime demand for women workers, and unprecedented increase in the number of women working. Yet, in 1955, the median wage and salary income of women who worked full time was still less than two-thirds that of men—almost exactly the same as it was in 1945. This suggests that it is not particularly important to a great many working women whether or not they earn as much as men, or have equal opportunities for training and promotion. What they seek first in work is an agreeable job that makes limited demands. Since they have little desire for a successful career in paid work, they are likely to drift into the traditional women's occupations. They are willing to become teachers, though they could earn more as engineers; willing to take factory and service and clerical jobs that hold little hope of substantial advancement (20, p. 108).

These observations by Smuts have been supported by many studies of adolescent girls and young women in high schools and colleges distributed widely over the country (3, 4, 18, 19). We might legitimately ask if there are implications we can validly derive from this particular change in the role patterns of one sex. Below, we will consider the data we have as they apply to the women themselves, to their husbands, and to their children, as far as it is possible and valid to separate the three.

From lengthy interviews with about forty working mothers,⁵ divided almost equally between professional and nonprofessional levels, the implication emerges that this change in sex role pattern is more a matter of form than function. Most of the working mothers we interviewed consider their work as an aspect of their nurturant function. It is another way in which they can serve their families. They do not substitute work for family obligations—they *add* it to the traditional roster of womanly duties and see it as another way of helping their husbands and providing for the needs of their children. Their husbands are still seen as the major and responsible bread-

⁵ These interviews took place as part of a study of children's concepts of women's social roles and were used to gather data concerning the background of the children who were subjects as well as to assess the attitudes of the interviewees concerning the satisfactions and frustrations of woman's role from the adult's point of view. Each interview took from 1½ to 2½ hours to administer.

winners—the mothers consider themselves merely as “helping” persons in this area.

The opportunity to help in this way has happy consequences for many. They feel a greater sense of freedom of choice, less coercion by blind fate, more integrity as persons, less frustration and less harassment by economic problems. The stimulation they receive from contact with others on the job and the change in physical surroundings between home and work even seem to make housework more satisfying and less like drudgery. The really bitter complaints about the hatefulness of household tasks tend to come from the nonworking women we interviewed rather than from those who worked at an outside job. That these salutary effects of the freedom to work may not be confined to our small sample is suggested by the findings of Hoffman, who in a study of 89 working mothers and 89 nonworking mothers, matched in pertinent variables, found that working mothers, in comparison to nonworking mothers, are warm, helpful, supportive and mild in their discipline, suggesting more relaxed and satisfied persons (9).

The freedom to work has, however, not been achieved without psychic penalty. The guilt some working mothers feel about their “self-indulgence” in going to work is marked and may have negative consequences to their daughters and to society at large. Typically, these “guilty” mothers do not work out of dire necessity; they enjoy their work and provide well for their absence from the household. Yet they seem to feel that there is something not quite legitimate about their preference for working and almost invariably give their children the impression that they work only because they need the money. “What other excuse would I have for working?” one such mother asked the interviewer. It seems to us that this situation reveals a questionable state of social values which induces women to feel that they have not a natural right to work at something congenial to them, which makes a positive contribution to the community, and which need not deprive their school-age children to any significant degree. That this anxiety is a socially induced one is suggested by Williams,⁶ who found that working mothers whose social contacts were mainly with others in a similar situation did not suffer these difficulties, while working mothers whose contacts were mainly with more traditionally oriented families experienced great discomfort. Perhaps as more and more mothers work, the social climate inducing these feelings will dissipate. Meanwhile, however, these attitudes are resulting in a strongly negative perception of the work situation in general on the part of many children (it is something one does only out of necessity) and a damaging lack of candor between parent and child. It is also, we feel, producing an ego-crippling effect on the female children involved, since it exerts a necessarily restrictive influence on the development of the child’s self-concept (6, 7, 24).

⁶From a preliminary statement of findings by Dr. Robin Williams in correspondence with the author.

Perhaps the most widely discussed area of concern related to this change in women's roles deals with its effect on men. The more sensational statements have implied the immediate emasculation of any male whose wife spends some time at money-earning. What does sober fact reveal? One traditional element of masculinity in our society is the dominance of the male in his family. Dominance implies control over other persons as well as control over possessions. Hoffman (9) suggests that control in the family situation might function in two ways: as *activity-control*, referring to control a person has over a given area of activities, regardless of whether or not this control has an important effect on others, and as *power*, defined as the degree to which one person makes decisions which control another person's behavior or makes decisions about objects which affect another person in an important way. In relation to activity-control, Hoffman found that working wives had less control and their husbands more than in families where wives did not work. In relation to the power component, no difference was found between working women and matched nonworking women. Although the husbands of the working wives may have participated more in household tasks after the women went to work than they had before, this apparently did not perceptibly affect the quality of their status in the family group.⁷

Actually, we still have very little reliable information concerning the effect of the woman's work role on the man's domestic role. Blood and Hamblin report that the husbands of working women participate in domestic activities to a greater extent than the husbands of nonworking women (1). Our own data, coming from children's perceptions rather than adult reports, suggest that the class variable is more significant in relation to male participation in traditionally female household tasks than the work status of the wife.⁸ Boys with working mothers assign domestic tasks to men more frequently than do boys with nonworking mothers ($p = .05$), it is true, but boys from working-class and lower-middle-class homes exceed boys from upper-middle-class homes by an even greater margin in making such assignment of roles ($p < .01$). This finding might suggest that assumption of the work role by women of lower economic strata might have more impact on traditional male roles than the same phenomenon at higher economic levels, where outside persons substitute for the wife in domestic tasks more frequently. Careful examination of our data, however, reveals that in the class comparison boys from homes with nonworking mothers contributed the bulk of the male domestic role assignments!

⁷ This opinion is also supported in the paper by Blood and Hamblin (1).

⁸ The raw data yielding these comparisons came from the responses of approximately 150 boys and girls, 5, 8, and 11 years of age, to the following statement: "Suppose you met a person from Mars and he knew nothing about the way we live here, and he asked you to tell him about girls (your age) in this world; what would you tell him girls need to know or be able to do?" These same questions were asked in relation to boys, women, and men.

It is interesting to note, in passing, that the boys in our sample were more prone to assign traditionally feminine domestic tasks to men than were the girls ($.2 > p > .1$). This may mean that these tasks are gradually being incorporated into the male self-concept, so that we may expect acceptance of an increasingly egalitarian division of all life tasks and a parallel diminution of rigid judgmental evaluations about the "manly" or "unmanly" nature of specific activities. A bit of clarification is necessary here, however. Male domestic involvement is regarded by our subjects as a "helping" role, with the major responsibility for household management and child care still unquestionably the woman's job. This feeling about male domestic participation echoes the feeling we have noted about the woman's participation in the work world: the activity is subordinate in importance both to the major responsibility of the sex involved and to the weight of the responsibility borne in the given area by the opposite sex. We are not witnessing an elimination of differences—only an amelioration.⁹

A few more items from the data collected in our study of the development of concepts of women's social roles might be of interest here. These deal with the apparent connections between a mother's work status and her children's concepts of the future roles of themselves and others. We found, for example, that the presence of a working mother made no difference in the assignment of nondomestic work roles to women by girls but was a significant variable (at the .05 level) with the boys. When we asked our female subjects about their own future plans, however, the work status of the mother was an important differentiating variable. Significantly more daughters of nonworking mothers mentioned housewife as their first choice of future occupation than did daughters of working mothers ($p < .05$). Significantly more daughters of working mothers said they thought they would work after they had children than did the others ($p < .05$). Similarly more daughters of working mothers tended to choose nontraditional vocations (among choices other than housewife) than did the daughters of nonworking mothers.

Despite the effect of working mothers on boys' assignment of work roles

⁹This amelioration is not characteristic of sex role patterns only—it is only one aspect of a pervasive social trend. Turning again to *Women and Work in America*, we find Smuts pointing out: "The increasing employment of middle-class, middle-aged wives is but one aspect of a series of developments that have tended to eliminate sharp differences in American society. . . .

"Rural and urban ways of life have come closer together. Many local and regional peculiarities have been all but erased as a result of increased industrialization. The vast gulf between the very rich and the very poor has been narrowed. Status distinctions among occupations have been blurred as the wages of manual labor have risen, and the brutalizing aspects of manual work in the last century have been abolished. The spread of free public education has reduced the cultural and economic advantages of the well-born. For present purposes, at least, the most important of the fading contrasts in American life is the contrast between the activities of men and women" (20, pp. 66-67).

to women, significantly more girls indicated plans to work after marriage than there were boys who said they expected their wives to work ($p < .01$). This disparity echoes a common finding (15) and indicates a force which we believe serves to slow the movement of women into the work world.¹⁰

Differences in plans for the future expressed by daughters of working mothers as compared with daughters of nonworking mothers may be explained in part by differences in their perception of the work role as such. This is suggested by the comparative proportions of children of working and nonworking mothers who made relatively positive and relatively negative interpretations of a pictured situation in which a woman was leaving her child to go to work.¹¹ Although about 64% of our subjects ($N = 108$) sensed some discomfort in this situation for the woman in the picture, only 37% of the children of working mothers interpreted her attitude as negative toward the work itself. In contrast, approximately 54% of the children of nonworking mothers thought that the woman must feel negative toward her work. These findings, combined with the others already described, suggest that the changes that have already taken place in sex role patterning will inevitably lead to extension of themselves, provided their orderly development is not interrupted by widespread social or economic crises.

The desirability of such extension may be a moot point. Some preliminary findings reported by Bronfenbrenner¹² from a study of family structure and personality development seem to be relevant here.

Studying the effects on children of parental absence, he gathered data from 450 students in the tenth grade of a small city and from their teachers. Parental absence was defined by a variable called "saliency" or the general extent to which a particular parent appears to be actively present in the child's world.

¹⁰This opinion was supported by materials collected from parents of our subjects. From interviews with 90 mothers, divided approximately equally among working and nonworking, we got the impression that the husband's attitude toward his wife's working often served as the deciding factor when neither economic necessity nor preschool children were involved—and some of the unhappiest women we talked with were among those who were prevented from working because there was no economic necessity for them to do so and their husbands could not conceive of any other valid reason. The unhappiest husbands we interviewed were those who aspired to upper-middle-class status, whose wives were compelled to work for economic reasons; their wives were not particularly unhappy about working.

¹¹The materials from which these data were derived were elicited in the following way. A picture showing a woman with a briefcase leaving the half-open door of a house through which a small child watched was presented to each subject. The interviewer said, "This little girl is at home and her mother is going to work. How does the mother feel about going to work? What makes you think so?" Other questions elicited reasons for the mother's working, alternatives to her working, and feelings of the child.

¹²Personal communication from Dr. Urie Bronfenbrenner giving a preliminary report of findings.

Bronfenbrenner found that the effect of parental absence on the child seems to depend on whether the missing parent is of the same or opposite sex and how much time the remaining parent spends in the home. The crucial person seems to be the parent of the same sex.

In relation to responsibility, for example, so long as the same-sex parent was present a good deal of the time, the child of either sex was rated as above average. If the same parent was in the high-absence group, and the opposite-sex parent was also absent a great deal, the responsibility rating dropped drastically. Similarly, if the remaining parent was in the high-presence category, the subject received a low rating. But if the same-sex parent was absent a great deal, and the opposite-sex parent present only an intermediate amount, the child received the highest mean rating on responsibility in the sample. Thus, if a boy's father was away from home a good deal, and his mother was either away excessively or present excessively, he tended to be rated low in responsibility. If the mother avoided either extreme, the son tended to be rated exceptionally high. Similarly for girls: if the mother was excessively absent, the relative presence or absence of the father seemed to determine the responsibility rating.

The implications of this study for our topic are intriguing. For one thing, it suggests that, in the traditional family set-up where the father is usually absent a great deal of the time, the continuous availability of the mother may be detrimental to her sons; they may be better served were the mother to be legitimately involved away from the home on a part-time basis. For girls, on the other hand, an optimal pattern would seem to be one in which the father is home somewhat more than ordinarily obtains, if the mother carries full-time responsibilities outside the home. The sex role pattern which seems to be currently increasing, with fathers fully employed and mothers employed part time, would seem to promise more for the development of boys than of girls. However, since the same-sex parent is still present a good deal of the time for girls, the latter seem not likely to be penalized.

Effects of parental presence and absence seem to differ markedly with differences in the socioeconomic status of the family, the father's level of education and his work orientation (whether quality oriented or not). The differential impacts of such variables as these may account for apparent inconsistencies in findings presented by studies focusing on different respective segments of the population. Thus we find many apparent contradictions between findings reported by the Gluecks (5) who studied mainly families of low socioeconomic status and those reported by Hoffman, Blood and Hamblin, and Bronfenbrenner, who either concentrated on middle- and upper-middle-class subjects or included a significant number of such subjects in their samples. Recognizing this state of affairs, we must limit any implications drawn from the data we have discussed to the type of population from which the data were collected. With this qualification in mind, a few general observations seem to be warranted.

The direction of current sex role change seems to be toward consistently greater egalitarianism, with leadership centered in families of higher educational levels. As the tendency toward the spread of higher education continues, we can expect more and more families to share this pattern. If psychologists are correct in assuming that one can give to others only what one has as part of the self, increased feelings of self-fulfillment and freedom of action which seem to accompany current developments in women's roles should lead to a lessening of crippling maternal possessiveness and a diminution of "momism."

The implications for men are less clear. It is unfortunately evident that current socialization techniques used with boys continue to saddle many of them with irrational and damaging anxieties about their ability to implement the male role (8, 11, 12). These anxieties are frequently accompanied, as one might expect, by extreme conceptual rigidities and limitations in self-definition and by a tendency toward defensive hostility. It is obvious that any change in the *status quo* would cause discomfort to such anxious individuals, and an increase in the apparent capability of females, whom they are taught at all cost to avoid emulating and whom they are pressured to best in order to validate their masculinity, would be particularly threatening. Perhaps we are approaching an era of more confident women and more anxious men. Frankly, this is pure speculation. It might be instructive to keep an eye on the sex ratio of male and female psychiatric referrals. At present they show a complete reversal between childhood and adulthood, with males far in the lead before adolescence and females coming out way ahead after adolescence (2, 14, 17, 21, 23). If significant changes in the relative proportions of satisfaction and threat impinging on each sex accompany changes in sex role patterns, the gap between female and male referrals should lessen.

There is one possibility of more generalized male difficulty which relates to ego-development in childhood rather than functioning in adulthood. Current socialization practices in relation to male children can create even greater difficulties than they do at present as the permissible role activities of girls widen. It is conceivable that the widespread use of negative sanctions, generally phrased in the unspecific and semantically confusing adjuration not to be a "sissy," may be responsible for more and more limitation in permissible activity for boys as girls enter into more and more formerly exclusively masculine activities (10, 16). There is some indication that in certain localities this is happening (22). However, this suggestion may be borrowing trouble. There is no indication at present that activities cease to be regarded as suitably masculine just because girls also engage in them, if they have been considered acceptable before girls joined the fun.

In conclusion, I should like to emphasize the following five points:

1. Concern about a possible increase in children's confusion about sex roles because of alleged changes seems to be without foundation. For one thing, children are not aware of the changes since they do not have the

time perspective for this. They react to the picture as they perceive it at any given moment, and changes in social roles are not so precipitous as to create contradictions from moment to moment. Let us beware of projecting our adult confusions on the children, and try to profit from them to create child-handling techniques that will ensure the necessary psychic flexibility that future developments will require.

2. Whatever changes have taken place seem to be mainly new means of fulfilling established and accepted functions and to imply no radical reversals of these.

3. There seems to be no realistic basis for the guilt many working mothers of school-age children feel about their work. Some of the evidence available at present suggests that it might, in fact, be desirable for some mothers to take part-time jobs.

4. There is no necessary implication of threat or damage in any perceptible current change in sex role activity. The real problem of adjustment to sex roles seems to be rooted in the differential pressures associated with respective developmental stages in each sex. Among males the pressures seem to be more exigent before adulthood; among females, during and after adolescence. In each case, special pressure toward limitation and restriction is identifiable during the periods for which comparative rates of breakdown are greatest for each sex.

5. In view of the above, current trends toward greater freedom of action for women would seem to be positive in implication, if the implied evaluative egalitarianism is also extended to the male sex. This seems to be taking place only in a very limited fashion, with the leadership centered in families of upper educational levels. Ego-strengthening socialization processes applied to male children seem to be of central importance for the smooth synchronization of change in sex role activity patterns.

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PATTERNS OF MOTHERING IN PSYCHOSOMATIC DISORDERS AND SEVERE EMOTIONAL DISTURBANCES

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The study to be reported represents one aspect of a more comprehensive research project concerned with the mother-child relationship in children who develop psychosomatic disorders. The basic hypothesis in this project is that there is a common denominator in all psychosomatic disorders: faulty caretaking by the mother in earliest infancy. Empirical observation led to the further hypothesis that the particular kind of relationship between mother and child is a close but mutually frustrating one. Previous studies (1) comparing the mother-child interaction in a group of children with psychosomatic disorders and that found in children with neurotic disturbances and in children with nonpsychosomatic physical illnesses revealed the following findings: Mothers of children with psychosomatic disorders were ambitious and controlling women who had high expectations for their children during pregnancy but derived little pleasure from caretaking activities with their infants; because of their strong emotional investment, however, they were irresistibly drawn to this ungratifying activity to the point of becoming entangled in a relationship highly charged with negative feelings and ungratifying to both mother and child.

A puzzling issue was left unsolved by these studies, however. Faulty caretaking during infancy which was hypothesized as underlying the development of psychosomatic disorders is also hypothesized, by psychoanalytic theory, as underlying severe emotional disturbances such as schizophrenia and the affective psychoses. It was of fundamental importance, therefore, to discover the differentiating features of a dynamic believed to be common both to psychosomatic and severe emotional disturbances.

A speculation by Gerard (2) served as a guide to making this differentiation. She wrote that "mothers of schizophrenic patients may be more disturbed, more rejecting generally than those of our psychosomatic cases,

so that more elements of adequate ego building are possible for those cases developing organ disorders. Emphasis of focus of the mother's rejection upon physiological functioning in the early months may be the differential etiological factor between schizophrenia and psychosomatic disorders." This speculation was formalized into a set of hypotheses which provided the conceptual framework of the empirical studies to be reported: First, the mothers of children who develop psychosomatic disorders are not as totally rejecting as mothers of children with severe emotional disturbances, and second, they focus their negative attitudes more specifically upon physiological functioning of the infant. In a like manner, children with psychosomatic disorders have less intensely negative feelings about receiving maternal affection and have more intact personality structures.

The population used in this study consisted of 26 mother-child pairs in the Psychosomatic group and 22 mother-child pairs in the Severely Disturbed group. The mean age of the children was approximately 8 years, with a range from 5-11 to 12-3. The groups were equated for intelligence, socioeconomic status, sex, race, and family intactness. The diseases included in the Psychosomatic group were bronchial asthma, rheumatoid arthritis, ulcerative colitis, peptic ulcer, and atopic eczema. A child was included in the Severely Disturbed group if at least one member of a diagnostic team of psychiatrist, psychologist, and social worker had found evidence of severe pathology. The resulting group contained few children who could be considered classically autistic or schizophrenic, but all were more disturbed than neurotic children.

A multitechnique approach was used to obtain data relevant to the hypotheses:

1. The first technique constituted the most direct approach to reconstructing the mother's attitude toward caretaking during infancy. It consisted of a semistructured interview conducted by a skilled social worker which explored, in some detail, the areas of pregnancy and infancy. The mothers were seen two to three times, and the interviews were recorded, transcribed, and edited so as to eliminate identifying data. Two trained judges rated this material in terms of 84 descriptive items. These items fell into four categories: Pregnancy ("this pregnancy was not wanted by the mother"); Child Training and Care ("this baby was comfortable in relation to feeding"; "mother seemed unusually disgusted by baby's stools or diaper changes"); Maternal Attitudes ("mother seemed unusually fearful and agitated in relation to baby"); and Physiological Status ("baby had excessive number of skin rashes"). On the basis of the hypotheses, it was predicted that the Severely Disturbed mothers would have a larger number of general maternal attitudes which were negative in nature, such as neglectfulness, inconsistency, and narcissism; on the other hand, it was predicted that the Psychosomatic group would be more negative in the specific child training and care activities, such as feeding and toileting.

Both predictions were clearly verified. Further analysis revealed that the Severely Disturbed mothers tended to be neglectful and to fail to provide adequate stimulation for their infants, rather than being ambitious and controlling. Another important finding was that the infants in the Severely Disturbed group were rated as having more signs of generalized physiological disturbance, such as excessive crying, restlessness, and hyperactivity, and that they engaged in more autoerotic activities. In the area of pregnancy, no new information was revealed beyond the already mentioned finding that the Psychosomatic mothers have high expectations for their children.

2. In the second technique, the mother told stories to a series of 20 specially selected photographs of children and adults in a variety of everyday child care situations. The major scoring of the stories focused on the affective interaction between parent and child. The principal categories were Positive Interaction, Negative Interaction, No Affective Interaction (i.e., no affect between parent and child or no parent-child relationship in the story), and Ungratifying Interaction (i.e., parent or child shows lack of appreciation for emotional state or actions of the other).

The results were as follows: Both groups of mothers rarely saw the interaction as one of mutual enjoyment or comforting closeness. The Severely Disturbed mothers, however, had the greatest number of nonaffective, noninteractive stories. Clinically, this blotting out of awareness of feeling seemed to be due to the fact that, when feeling was introduced, it was intense and traumatic—a crying child was seen as starving, or terrorized, or abandoned on a doorstep; a child was not playing peek-a-boo, but had just lost an eye. By contrast, the Psychosomatic group had the fewest ratings on the No Affective Interaction category, but the interaction was negative and ungratifying. The mothers were highly aware of the children's feelings but ministered to them in intellectual, ambivalent, manipulative, or overtly negative ways.

3. The third technique consisted of direct observation of the mother-child interaction. The mother was told that the examiner was interested in observing how her child played with a certain construction toy. She was to bring the toy to the child and remain with him while he played, but she would be free to do whatever she liked. Three judges observed the subsequent mother-child interaction and, immediately afterwards, rated it on a scale for the following characteristics: the atmosphere of the interaction (e.g., positive closeness, comfort, negative closeness); the techniques of interaction (e.g., domination, competition, encapsulation); and individual personality characteristics such as anxiety and narcissism.

The following results were obtained: The Severely Disturbed group represented an extreme in lack of positive closeness, in discomfort and destructiveness, and in negative interaction. The mother was rated as extremely self-centered and lacking in child-centeredness and empathy; the

child showed a marked degree of encapsulation and few signs of irritability and anger with the mother. In the Psychosomatic group, domination, discomfort, and competition were the outstanding features; the child showed a good deal of overt anger with the mother but was lacking in independence and was victimized by her. The two types of interactions can be conceptualized as follows: In the Severely Disturbed group there is both a maximum negative interaction and a maximum obliviousness; the mother, in particular, is impervious to the effects of her behavior on the child or the ideas and feelings the child is trying to communicate. In the Psychosomatic group, the picture is one of negative entanglement of mother and child, with an irresistible temptation to domination, control, and victimization.

4. The final techniques to be discussed were used to obtain the children's fantasies about being mothered and to measure the extent of psychological disturbance. The first was a series of 10 pictures, 5 photographs of mothers and children in close, positive relationship and 5 drawings of witches and children in uncommon fairy tale settings. The children were instructed to tell a story to each. These stories were subsequently scored for the kind of mother-child interaction which was introduced into the realistic photographs and the amount of traumatic content fantasied to the witch pictures. The second technique was the World Test which was used both to stimulate fantasy and as a measure of psychological intactness.

As for results: In their stories, both groups of children pictured the mother as lacking in maternal feeling, although this was more extreme in the case of the Severely Disturbed group. The Psychosomatic children had two characteristic kinds of negative interaction which took place within the general framework of destructive closeness—one in which the mother was depicted as either manipulating and controlling, and the other in which she unpredictably swung from positive to negative feelings. The Psychosomatic children also introduced the greatest amount of traumatic theme into the witch pictures. Surprisingly, the Severely Disturbed group revealed no characteristic negative feelings; on the realistic pictures, on the witch pictures, and in the World Test, the striking feature was the blocking which was specific to the mother figure. The intensity of their hostility came out, however, in the large number of generally aggressive and destructive themes on the World Test (e.g., wild animals tearing down the walls of a town). One can speculate that the Psychosomatic children have more outlets for expressing their negative feelings toward the mother in direct and indirect ways; the Severely Disturbed children are quite blocked in this respect and must displace their aggression on to nonmaternal and even nonhuman figures.

As could be expected, the World Test revealed more signs of pathology and more extreme pathology in the Severely Disturbed group. There was significantly more distortion suggesting senseless ritualistic behavior and

bizarre ideation, and there was significantly more aggression. These findings take on added significance when viewed in light of the controversy over the degree of disturbance in individuals with psychosomatic disorders. Some investigators claim that such disorders represent a defense against psychosis or alternate with psychosis. The present study fails to support this notion, and there is no evidence for the proximity of psychosomatic disorders to psychosis in terms of degree of pathology.

We will now review the findings in light of the hypotheses. In line with the first hypothesis, there is evidence that the mothers in the Psychosomatic group derive little pleasure from the actual caretaking activities with their infants, while the mothers in the Severely Disturbed group have more pervasive negative attitudes. However, there is also evidence that, as an infant, the Severely Disturbed child was restless, irritable, and preoccupied with autoerotic activities, and the present data shed no light on the vexing question of whether the direction of pathology was from mother to infant, or from infant to mother. In line with the second hypothesis, there is evidence that the Psychosomatic children have more intact personality structures than do the Severely Disturbed children. Whether the Psychosomatic children have less intensely negative feelings about receiving maternal affection depends, to a certain degree, on how the empirical findings are interpreted. Certainly, both groups of children feel the lack of maternal affection, the Severely Disturbed ones somewhat more than the Psychosomatic. However, the Psychosomatic child can express the ensuing hostility in aggressive acts or fantasies; the Severely Disturbed child is blocked, must retreat into protective encapsulation, and becomes preoccupied with fantasies of general destruction.

There is evidence from all techniques that the mother-child interaction is more negative in the Severely Disturbed group, but the hypothesized "rejection" does not epitomize the relationship. The hints of neglect and inadequate stimulation of the infant, the obliviousness to feeling in the fantasy material, and the self-centeredness and lack of positive feeling in the observed interaction form a consistent picture of a mother who perceives the parent-child interaction as affectively blank. Once the basic avenue of awareness is blocked, many kinds of faulty maternal behavior can result, and there is evidence that the mothers behave with great destructiveness even though their particular techniques vary. The essential feature of this behavior, however, is an insensitivity, and inappropriateness, and its destructiveness is rooted in the mother's obliviousness to the signals the child sends and the effects of her behavior upon him. The Psychosomatic mother is also unable to enjoy her child and is uncomfortable about emotions. She handles this discomfort, however, not by denying that feelings exist, but by attempting to gain control over them. She is aware of the child's feelings but uses this awareness to dominate and control. Mother and child become entangled in a mutually frustrating relationship, each

aware of the other's vulnerability and each irresistibly impelled to use this awareness in an attempt to gain power over the other.

In the Severely Disturbed group, therefore, imperviousness is the hallmark of the mother and inappropriateness characterizes the interaction; in the Psychosomatic group, domination and control characterize the mother and a relentless power struggle typifies the interaction.

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RELATIONSHIP OF PHYSICAL TRAITS AND MENTAL TRAITS IN INTELLECTUALLY GIFTED AND MENTALLY RETARDED BOYS

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The study of the relationship between physical traits and intellectual traits has had a long and provocative history. In a review of the literature in 1930, Patterson (9) concluded that the degree of demonstrable relationship was too small to be of practical usefulness. His conclusion seemed to be strong enough and final enough to terminate serious discussion of the matter. This was not to be the case. In 1937, Olson (7) drew attention to observations of concomitant variations in growth based on longitudinal studies then in progress at the University of California, the State University of Iowa, Harvard University, and the University of Michigan. Later Olson and Hughes (6) presented evidence in support of the hypothesis that the individual child tends to maintain through time a dynamic interrelationship of various attributes of physical, intellectual, emotional, and social growth around an individual center of gravity. To the center of gravity they gave the name "organismic age." The data used were from longitudinal records of children who attended the University School in Ann Arbor, Michigan.

A number of investigators have rejected the organismic age concept. Bloomers, Knief, and Stroud (1) sought to determine whether physical growth is related to mental growth and to academic achievement. Using data collected by Choitz (2) on groups of approximately 40 fourth, fifth, and sixth graders, they found that the addition of height age, weight age, and dental age to mental age raised the correlation coefficient between reading achievement and mental age and arithmetic achievement and mental age by only approximately .01. Furthermore, an organismic age variable based on the average of mental age, weight age, and dental age accounted for an insignificant percentage of variation among the three separate measures within individual pupils. They conclude that

Since it is obvious, . . . that the children in a grade do differ with respect to single growth characteristics, the fact that these same children do not differ significantly in terms of the average of a number of growth characteristics is evidence of nothing more than the chance association of individual growth measures within an individual child. There is no systematic tendency, in other words, for a child advanced in mental age to be advanced also in weight age or in dental age (1, p. 148).

Klausmeier, Beeman, and Lehman (5) have presented findings which are in accord with those of Bloomers, Knief, and Stroud. A comparison was made of the efficiency of organismic age and regression equations, based on raw scores, in predicting arithmetic and language scores 12 months later. The regression equation predictions showed a more significant correlation with arithmetic and language achievement than did organismic age predictions.

Olson (8) has recognized the small coefficients of correlation which result when measurements of physical and mental traits are correlated. Nevertheless, he has hypothesized that large differences in achievement are related to differences in physical traits, provided the extremes for the physical traits are used. His support for the hypothesis rests on the following discussion:

Let us examine the significance at the extremes of the often disparaged coefficient of .30 in the growing period between physical and mental traits. With this correlation, persons who are two S.D. and more below the mean on the physical trait will be .6 S.D. or more below the mean on the mental trait. Let us take an S.D. of 19 months at age 120 months as a sample of intellectual achievement. Six-tenths of 19 months is 11.4 months. The average reading age for a group of boys at exactly 2 S.D. above the mean in a physical trait is 127 plus 11.4 or 138.4, while a group the same distance below the mean is 115.6. Thus the presence of the small correlation of .30 gives a separation of 22.8 months or almost two years' difference in achievement at the extremes of the physical dimensions (p. 219).

The purpose of the present study was to investigate the relationship of physical and mental traits in growing children by using the extremes in distributions of the traits as suggested by Olson. The hypothesis formulated was that when the extremes are used, children advanced in mental growth are advanced in physical growth and children retarded in mental growth are retarded in physical growth.

PROCEDURE AND SUBJECTS

Ketcham (3) has shown that children with I.Q.'s above 130 who attend the University School of the University of Michigan are advanced beyond their chronological age norms for all physical and achievement measures. Solomon (10) has shown the opposite trend for the same measures for mentally retarded boys of the endogenous type at the Wayne County Training School at Northville, Michigan. Subjects for the present study were selected at random from these studies: 25 gifted boys and 25 mentally retarded boys.

RESULTS

Data consisted of measures of the height, weight, reading skill, and arithmetic skill of the subjects at 120, 132, 144, 156, and 168 months of

chronological age and an I.Q. at 120 months. Tests used were the Stanford Revision of the Binet Scale for I.Q.'s and the Stanford Achievement Battery for reading and arithmetic skills. Measures for height, weight, reading, and arithmetic were converted into age units in months. This provided an effective method for comparing the growth ages of the two groups at a given chronological age.

Differences in intelligence and reading and arithmetic skills for the gifted and retarded subjects of this study are shown in Table 1. The data reveal the extent to which the two groups represent extremes in ability and achievement. According to available data on the distribution of I.Q.'s (11) the intelligence of the gifted group is equal to the top four per cent of the general population, while the intelligence of the retarded group is equal to the bottom 11 per cent. The latter figure tends to give the impression that the intelligence of the retarded group is less extreme than that of the gifted group. However, the elimination of the two highest I.Q.'s (80 and 76) would make the remainder of the retardates' I.Q.'s equal to the bottom four per cent of the general population and comparable to those of the gifted group with respect to the degree of deviation from the general population mean. The retarded group shows achievement ages equal to those of the lower five per cent of the general population. Achievement data for the gifted do not show a comparable degree of upward extremeness. However, by chronological age 156 months the achievement ages of all were well above their chronological age; and, with the exception of five subjects for reading and three subjects for arithmetic, all were achieving at or better than the 75th percentile for the general population. At chronological age 168 months, 11 of the gifted boys were within the range of the top five per cent of the general population for both reading and arithmetic. The mean reading ages for the two groups are six years, six months apart at age 10; and 10 years, eight months apart at age 14.

TABLE 1

COMPARISON OF LONGITUDINAL RECORDS OF MEAN READING AGE AND MEAN ARITHMETIC AGE AT COMMON CHRONOLOGICAL AGES IN MONTHS AND MEAN STANFORD-BINET IQ AT AGE 120 MONTHS FOR GIFTED AND RETARDED BOYS (N = 25 IN EACH GROUP)

Chronological Age	I.Q.			Reading Age			Arithmetic Age		
	Gifted	Ret.	Diff.	Gifted	Ret.	Diff.	Gifted	Ret.	Diff.
120	149	66	83	155	77	78	142	81	61
132				178	77	101	164	85	79
144				195	81	114	186	91	95
156				209	86	123	206	93	113
168				220	92	128	224	98	126

The differences in the means for arithmetic are five years, one month at age 10; and 10 years, six months at age 14.

The total range from the bottom of the retarded group to the top of the gifted group is large for the three measurements. The difference in I.Q. between the least bright boy among the retarded and the brightest boy among the gifted is 114 points. The ranges from the lowest achiever in the retarded group to the highest achiever in the gifted group at age ten are 11 years, six months for reading and 10 years, one month for arithmetic.

Mean differences in intelligence and achievement of the magnitude presented in Table 1 are representative of the extremes in the general population. The question under investigation is whether the groups are also differentiated to a comparable degree with respect to physical traits. Data from the longitudinal records of height ages and weight ages are summarized in Table 2. The mean differences for height age range from 20 months at age 10 years to 32 months at age 14 years. For weight age the mean differences at the same chronological ages range from 22 months to 32 months. All of the differences are statistically significant beyond the .01 level. The mean differences between the two groups for mental traits are smaller than the mean differences for physical traits. This is to be expected, since the variation for mental traits is much larger than the variation for physical traits among growing children and adolescents. For example, the standard deviation for reading age among 12-year-olds is 30.4 months as compared with a standard deviation of 18.4 months for height age (4). Differences in physical traits between the two groups are illustrated further by a comparison at 120 months of the shortest retarded boy with a height age of 85 months and the tallest gifted boy with a height age of 157 months: a difference of six years.

Another view of the extent of differences with respect to growth in height is presented in Figure 1. It is clearly shown that, although the

TABLE 2

COMPARISON OF LONGITUDINAL RECORDS OF MEAN HEIGHT AGE AND MEAN WEIGHT AGE OF GIFTED AND RETARDED BOYS AT COMMON CHRONOLOGICAL AGES IN MONTHS (N = 25 IN EACH GROUP)

Chrono- logical Age	Mean Height Age			Mean Weight Age		
	Gifted	Ret.	Diff.	Gifted	Ret.	Diff.
120	134	114	20*	138	116	22*
132	145	123	22*	150	127	23*
144	159	134	25*	164	136	28*
156	174	145	29*	178	149	29*
168	189	157	32*	194	162	32*

* Significant beyond the .01 level.

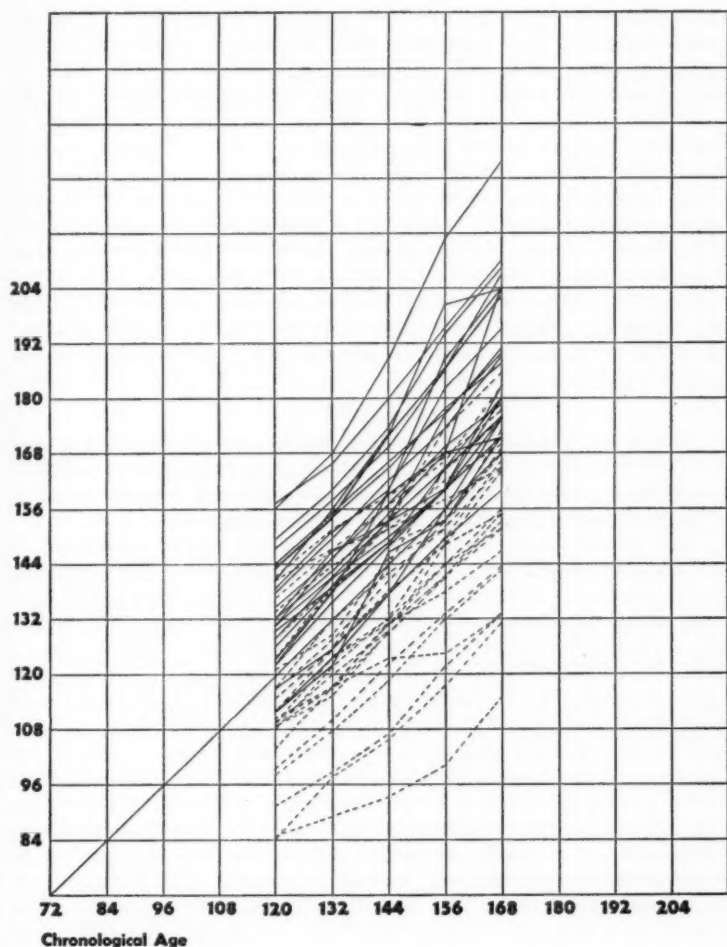


Fig. 1. Comparison of Longitudinal Records of Height Ages for Gifted (—) and Retarded (-----) Boys.

differences between the mean height ages for the retarded and gifted subjects are statistically significant, there is an overlap at all chronological ages. However, when the growth curves for height age are translated into slope

values which indicate rate of growth, a subtle difference between the retarded and gifted boys is revealed. The mean rate in the development of height age over the four-year period was .89 for the retarded and 1.16 for the gifted. The mean difference of .27 is significant beyond the .01 level. This difference in growth rate for height produced an increase in differences between the two groups through time. At 120 months, seven of the retarded group had height ages greater than 120 months while three of the gifted group had height ages less than 120 months. At 168 months, only one gifted boy had a height age under 168 months, compared to 20 of the retarded subjects. At 120 months, six of the gifted boys were taller than any of the retarded. By 168 months, the number had increased to thirteen. This finding is consistent with the increase in mean height age and weight age differences presented in Table 2. A very similar pattern of increasing differences in mean reading age and mean arithmetic age is evident from Table 1.

DISCUSSION

The results of this study revealed that various physical and mental traits are interrelated in growing children when subjects at the general population extremes for mental traits are compared with regard to physical traits. Important implications for education are suggested.

The present study suggests that great caution must be exercised in predicting the intellectual ability or academic achievement of individual pupils from their physical traits. Nevertheless, it suggests that fundamental differences in intelligence and in school achievement for groups of children can be predicted from knowledge of their physical size and growth. Expectations for the results of various administrative practices and teaching and remedial techniques can be established by considering both the physical and mental traits of individuals. The special importance of the physical traits is derived from their predominately genetic origin. Teachers frequently attempt to raise the intellectual and academic status of pupils while ignoring probabilities for increase which growth usually provides. Like policies do not prevail with regard to either status or growth in physical size. It is widely recognized that physical size is dependent on adequate nurture, but elaborate programs designed to add or subtract from the ultimate goal provided by heredity are practically nonexistent. The present study suggests, however, that both the physical and the mental traits of groups of children show a strong common origin. The implication for education is that group differences in school achievement receive important added explanation by considering differences in the physical traits of growing children.

Many educational problems center around the nature and magnitude of individual differences in academic achievement. Teaching is frequently viewed as a process of eliminating individual differences either through teaching techniques or administrative practices. Analysis of the longitudinal

data in this study has revealed that both physical and mental traits in growing children show a high degree of congruence in their tendency to expand with increases in chronological age. This suggests that the generally observable increases in variability in academic achievement among children is a fact of nature and not a result of education.

SUMMARY

The relationship of physical and mental traits in groups of mentally retarded and gifted boys was studied. The subjects were selected from the extremes for the general population with regard to mental traits, i.e., I.Q., reading age, and arithmetic age. Physical traits, i.e., height age and weight age, were found to exhibit comparable degrees of extremeness for the sample studied. The results of the study reveal strong evidence for interrelationships between physical and mental traits among children who grow at the extremes for the general population.

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COGNITION, CATHEXIS, AND THE "DYADIC EFFECT" IN MEN'S SELF-DISCLOSING BEHAVIOR¹

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One can be associated with other people for long intervals, as in work-settings, and never come to know these others as individual *persons*. To know another man as a person means, among other things, to know idiosyncratic, ordinarily private data about him, the kinds of information which are most readily learned through his confiding, or verbal self-disclosure. Since verbal self-disclosure is voluntary—a variety of operant behavior (9, pp. 185-226)—it is evident that people can control the degree to which they will become known as "selves" by regulating the amount and content of their self-disclosure to others.

Self-disclosure plays a crucial role in psychotherapy and in intimate interpersonal relationships such as those between spouses or between parents and children, and thus basic knowledge about its conditions and dimensions is much needed. However, with the exception of practical tips from interviewers and psychotherapists about effective means of fostering free disclosure in patients and respondents—viz., permissiveness, "reflecting"—not much basic information is known about this crucial class of interpersonal behavior.

As an attempt to identify some of the factors in self-disclosure, Jourard and Lasakow (5) undertook some preliminary explorations with a self-disclosure questionnaire. This instrument listed items of personal information classified into several categories; subjects (Ss) responded by indicating on an answer sheet whether or not they had revealed this information to each of several "target-persons," or confidants. Jourard and Lasakow (5, 2) found that unmarried college subjects disclosed some kinds of information more than others; white college students were higher total disclosers than Negro students; women were higher total disclosers than men; mothers were confided to more than fathers; and same-sex friends were disclosed to more than opposite-sex friends. A subsample of female Ss in that study was tested with a "parent-cathexis" questionnaire (1), and significant correlations were found between mother- and father-cathexis and the

¹ Adapted from a paper presented at the meetings of the Southeastern Psychological Association, St. Augustine, Florida, 1959.

amounts disclosed to those "target-persons." It was concluded that cathexis was a further factor in disclosure, at least among females.

In a subsequent investigation, Jourard (4) found significant correlations between the scores measuring nursing college faculty members' degree of liking for each of eight colleagues and the amount disclosed to each colleague. These data provided stronger support for the hypothesis that cathexis was a factor in disclosure among females. It was further found that the Ss tended to establish dyads of reciprocal intimacy: they disclosed most to those colleagues who most confided in them, and vice versa.

The present study sought to determine the degree to which men varied self-disclosure to available confidants with liking for those persons, to ascertain whether knowledge of others was a closer correlate of disclosure among males than liking, and to determine whether the males, like females, would show a dyadic effect in their disclosing patterns.

HYPOTHESES

The hypotheses tested were as follows:

1. Men will vary the amount of personal information they disclose to other men according to the degree they like the others.
2. Men will vary the amount they disclose to other men according to the degree they know the others.
3. The correlation between knowing and disclosure among men will be higher than the correlation between liking and disclosure.
4. Men will show a "dyadic effect" in their disclosure pattern, i.e., they will disclose the most to men who have disclosed the most to them, and vice versa.

METHOD

Subjects

Nine male graduate students enrolled for a year or longer in the University of Florida Department of Psychology were selected as Ss from among a larger group of about 40 students. At the time the data were gathered, 5 Ss were married, and 4 were single. Mean age was 28 years, with a range from 23 to 32.

Disclosure-Output and -Intake Scores

Each S was interviewed by the junior author to ascertain the information called for by the questionnaire shown as Table 1. Then, each S indicated to which of his 8 fellows he had revealed each item of information. Finally, each S was asked what information from the questionnaire he knew about each of his fellows. The number of items which each S told his

TABLE 1
THE SELF-DISCLOSURE QUESTIONNAIRE

1. When you were an undergraduate, did you participate in any extracurricular activities, e.g., clubs, dramatics, sports, etc.?
2. In what town were you born?
3. Have you ever gone steady or been engaged? If so, what was her (their) name(s)? OR In what year were you married?
4. Do you have any brothers or sisters? How many of each?
5. Do you have a main hobby or interest(s) outside of school? What?
6. What physical malady(ies) bother you at present, recurrently (if any)?
7. What characteristics of yourself bother you most?
8. Do you go to any church in town?
9. What aspect(s) of your physical appearance do you regard as your chief problem, that you dislike most, that you wish you could change or improve?
10. What is the name of the teacher whom you count as outstanding and most influential during your training?
11. Are there any sports you engage in at present or when there is an opportunity?
12. Is there any food or dish that you especially dislike?
13. With which faculty member do you find it most difficult to get along?
14. What is your usual monthly income (within \$25)?
15. What is or was your father's chief occupation?

fellow students constituted his disclosure-output scores; the number of items disclosed to each S by his fellows, his -intake scores.

The validity of the disclosure-output and -intake scores was checked by comparing what each S knew about each fellow with the information disclosed by the latter to the examiner. The output and input scores may be deemed valid, since there was close coincidence between what each S knew about his fellows, what he said he disclosed to his fellows, and what each S told the examiner about himself.

Liking and Knowing Scores

Each S was then given two questionnaires listing the names of his fellow students arranged as paired-associates. One of these was completed according to the instruction: "Like better of the two"; and the other, according to the instruction: "Know better of the two." These procedures resulted in a forced ranking of his fellow students by each S. The ranks were called, respectively, *liking* and *knowing* scores. All 9 Ss were ranked as well from best to least liked, and best to least known, on the basis of the liking and knowing scores.

RESULTS

Liking and Disclosure-Output

Column 5 in Table 2 shows rank-order correlations between the liking rank to which each S assigned his fellow students and the rank for the

TABLE 2
RANK-ORDER CORRELATIONS (Rho) BETWEEN KNOWING OTHERS, LIKING OTHERS, AND DISCLOSURE

S's		RANK-ORDER		TOTAL ITEMS DISCLOSED		RHO							
		Cathexis by colleagues†	Known by colleagues	By S	To S	Items disclosed BY S vs. S's		Items disclosed TO S vs. S's		Disclosure-output vs. -intake	Liking vs. knowing	Disclosure-output vs. -intake	
						Liking of colleagues	Knowing of colleagues	Liking of colleagues	Knowing of colleagues			Liking partialled out	Knowing partialled out
1	2	3	4	5	6	7	8	9	10	11	12		
Al	1	4	33	34	.81*	.81*	.90**	.86**	.87**	.93**	.54	.57	
Bert	2	6	21	34	.35	.95**	.28	.87**	.77*	.14	.74*	.38	
Cal	3	9	13	22	.56	.79*	.38	.79*	.80*	.46	.77*	.47	
Dan	4	7	53	38	.33	.81*	.60	.96**	.74*	.72*	.72*	.25	
Ed	5	2	35	34	.49	.90**	.21	.79*	.70*	.57	.87**	.04	
Fred	6	1	43	38	.49	.86**	.57	.48	.79*	.72*	.72*	.84**	
Gus	7	5	31	20	.69*	.79*	.55	.59	.36	.81*	.03	.22	
Hal	8	8	16	19	.61	.70*	.45	.81*	.65*	.74*	.54	.20	
Irv	9	3	31	37	.62	.46	.67*	.46	.71*	.93**	.50	.63	

*Significant at the .05 level.

**Significant at the .01 level.

†In order of decreasing cathexis.

amount he disclosed to them. Significant ρ 's were found in only 2 out of 9 cases. The males thus showed only a slight tendency to vary disclosure-output to others with liking for them.

Knowing and Disclosure-Output

Column 6 in Table 2 shows significant ρ 's in 8 out of 9 cases between knowing and disclosure-output. Clearly, these Ss showed a strong tendency to disclose most about themselves to the fellow students whom they knew best and least to those whom they knew less well.

Knowing Versus Liking as Factors in Disclosure-Output

Comparison of the ρ 's between liking and disclosure (Column 5) and knowing and disclosure (Column 6) shows that in 7 cases the knowing-disclosure correlation was the larger; in 1 case it was smaller; and the ρ 's were identical in another instance. By the signs test (8, pp. 68-75), the probability of getting 7 out of 8 larger ρ 's is .035. It may be concluded that the correlation between knowing and disclosure was stronger than that between liking and disclosure.

Disclosure-Intake as a Factor in Disclosure-Output: The Dyadic Effect

Eight of the 9 Ss showed significant correlations between the amount they disclosed to each of their fellow students and the amount that their fellows disclosed to them (See Column 9). This finding shows that disclosure was a reciprocal kind of behavior which proceeded to a level of intimacy agreeable to both parties in each possible dyad and then stopped.

Since liking and knowing were differentially correlated with disclosure, it was pertinent to determine what happened to the dyadic effect when liking and knowing were statistically controlled. Accordingly, the correlations between disclosure-output and disclosure-intake were recomputed, with liking (Column 11) and knowing (Column 12) partialled out. When liking was thus controlled, there were still 5 out of 9 significant correlations remaining, whereas only 1 out of 9 ρ 's remained significant when knowing was partialled out. This comparison lends further support to the proposition that liking was not as strong a factor in self-disclosing behavior among these males as knowing.

FURTHER FINDINGS

The findings reported above supported the hypotheses proposed in an earlier section. Certain other analyses, not directly bearing on these hypotheses were conducted, and they are reported below.

Liking and Disclosure-Intake

If a man likes another, does it necessarily follow that the other person will then confide in him? Column 7 shows the ρ 's between liking and the amount of confiding which was directed at each S by his fellows. Sig-

nificant ρ 's were obtained in only 2 of the 9 comparisons. It may be concluded that an S's liking for each of his fellows was not a strong determiner of their disclosures to him.

Knowing and Disclosure-Intake

Significant ρ 's between knowing and disclosure-intake were found in 6 out of 9 cases (See Column 8). This shows that the men tended to receive the most disclosures from those whom they knew best, or more probably, that a given S's degree of knowledge about others was the outcome of being disclosed to. Stated another way, there appears to be nothing magical about coming to know another person. If he discloses himself to you, you will know that much about him, for a certainty, and no more. This finding has definite implications for psychotherapy, as well as for interpersonal relationships in general: People who wish to become known and understood must disclose themselves.

Sociometric Status and Self-Disclosure

The Ss named in Table 2 are listed in descending order of likability to their fellows—a crude type of sociometric status-ranking. The question may be raised: Is there any relationship between the degree to which a person is liked by his fellows and his average disclosure-output? A ρ of .05 between likability rank and disclosure-output rank, which was not significant, shows that in this group, likability and disclosure-output were not related.

A rank-order correlation of .12, which was not significant, was found between likability rank and rank for total disclosure-intake. This indicates that the persons in the group who were best liked by their fellows were not necessarily confided in more than the Ss who were the least liked.

It may be concluded that likability to one's fellows was not systematically related either to disclosure-output to one's fellows or disclosure-intake from one's fellows.

Self-Disclosure and Degree Known

Ranks for the degree to which each S was known by his 8 fellows are shown in Column 2 of Table 2. These ranks were found to correlate .62 ($P < .05$) with rank for total disclosure-output and .54 with rank for total disclosure-intake. The latter ρ was not significant. The fact that degree known was correlated significantly with total disclosure-output serves both as an index of the validity of the self-disclosure questionnaire and of the more obvious fact that the way one becomes known by one's fellows in a group is through self-disclosure.

Relationship of Liking to Knowing

Though 6 of the 9 Ss showed significant ρ 's between liking and knowing, it does not mean that the Ss liked whom they knew. Common sense tells us that knowledge about someone may provide the basis for dislike.

Moreover, when we correlated the likability ranks of the 9 Ss with their ranks for degree known, a low (insignificant) ρ of $-.20$ was obtained. If this ρ had reached significance, it would mean that there was a slight tendency for those Ss who were best known to be the least liked.

DISCUSSION

The present findings showed that liking for another was not as strong a determiner of men's disclosing behavior as either the degree to which a man knew each of his fellows or the extent to which each of the others had confided in him. Since knowing was found to be related to disclosure-intake, it seems likely that each of the 36 possible dyads in the group commenced mutual confiding up to a point where the Ss knew as much as they wanted to know about one another and then stopped. These data did not afford any leads as to why confiding stopped at the point it did.

The relatively minor role played by cathexis in men's disclosure contrasts sharply with the major role it played in the disclosure patterns of the female sample studied earlier (4). The men and women alike showed a marked dyadic effect—the men's slightly stronger than the women's—but the strong association between liking and disclosure was peculiar to the females.

There is reason to suppose that this sexually differentiated response to interpersonal cathexis is related to the social definition of the male and female role. Women in most societies are trained to assume "expressive" roles in most of the social systems in which they participate. The expressive role calls for especial concern with, and responsiveness to, feelings. Men, trained mainly to adopt "instrumental roles," doubtless are encouraged to suppress or distrust feelings and to base their transactions with people on cognitive factors rather than emotion (6, pp. 317-318). Certainly, there is some evidence to support this interpretation. Earlier study of self-disclosure (5) showed that women were higher total disclosers than men.² This sex difference was further supported by comparison of the mean total amount of disclosure shown by the nursing faculty (4) with that found in the present study. Although the groups were not entirely comparable (since they differed in mean age, in marital status, occupation, etc.), the testing procedures were identical; the nursing faculty members disclosed a mean total of 48.22 items (S.D. 4.60) compared with a mean total of

² Rickers-Ovsiankina and Kusmin (7) reported some data on sex differences in disclosure which conflict with our observations. They employed a "social accessibility" questionnaire which asked Ss to indicate their *willingness* to confide items of personal information if they were asked by (a) a stranger they would never see again, (b) an acquaintance, and (c) their best friend. The males had a slightly higher total score for willingness to confide than the females. It is not known whether this discrepancy with our findings stems from differences between the samples tested, between questionnaire items, or differences in the set which Ss assumed in responding.

30.67 (S.D. 10.98) for the males. The difference between means was significant at the .02 level ($t = 2.72$).

Tentatively, it may be proposed that men follow their role definitions when they keep their "selves" to themselves, confiding in someone only after they have gotten to know enough about him to trust him. Obviously, the man can know the other person only after receiving some disclosures from him. The dyadic effect noted above suggests that disclosure proceeds on a *quid pro quo* basis—"you tell me and I'll tell you." Doubtless it is an occasion of anxiety for many men if they have revealed more about themselves to another man than he has revealed to them. Perhaps this is why the unilateral disclosure which occurs in psychotherapy is so threatening and is so often resisted by the patient: The therapist knows so much more about the patient than the latter knows about the therapist (3).

The fact that the men have been found to be lower disclosers than the women and that they tend to discount feelings as a basis for disclosure seems to corroborate not only role theory but also statements abounding in popular literature which portray men as strong and silent and distrustful of feelings; women are commonly portrayed as more emotional, talkative, overly trusting of those whom they like, and hence more vulnerable to betrayal than men.

SUMMARY

Nine male graduate students were tested with a brief self-disclosure questionnaire and questionnaires measuring degree of liking and degree to which each knew each of his fellow students. The amount of personal information which the Ss revealed to their fellows was highly correlated with the degree to which they knew the others and the amount the others had disclosed to them. Liking was only slightly correlated with disclosure within this male sample.

Additional analyses showed that there was only a slight relationship between a given S's liking for fellow students and the latter's disclosures to him. The degree to which each S knew his fellows was highly correlated with disclosure-intake from them. Average likability within the group (sociometric status) was not correlated either with S's total disclosure-output or -intake. The average degree to which an S was known by his fellows was correlated with his total disclosure-output but not significantly with his total disclosure-intake.

The males of this sample disclosed significantly less than did a sample of nursing college faculty who had undergone identical testing.

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EVALUATING FAMILY LIFE EDUCATION¹

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When one of Shakespeare's alter egos remarks, "Sweet are the uses of adversity," he is *not* discussing current criticisms of the family life education movement. However, these criticisms—some profound, and some panicky—have produced several healthy activities among family life educators. An aroused interest in measuring the results of family education will hopefully be one of these activities. In this paper a review of some of the general problems found in such evaluation research will be made, and current efforts to evaluate family education at one academic institution will be reported.

Although evaluation research is an old endeavor among educators, it is only within the past few decades that organized research efforts with careful methodologies have been utilized for this purpose. The rationale behind such research has been that in a pragmatic, business-oriented culture faith alone is not enough to substantiate education or any other action program. For example, after reviewing twenty-three studies of parent education programs, Brim has remarked: "It is perhaps disappointing that there exist for the far-reaching and multimillion dollar activity called parent education this mere handful of efforts, many of them crude indeed, to discover whether this activity has any effect at all" (1, p. 56; 2).

Not everyone, however, accepts this rationale. And the first problem that arises in the field of evaluation research is: Should it exist? From some philosophical starting points, evaluation research is rejected as being too much a case of an "outer" source measuring something which should only be measured by "self."

Other critics pass by this philosophical dilemma and point to its possible ethical outcome. That is, they note that evaluation research can be used as the tool of administrators and can create in individuals a debilitating fear of being compared with others or with a standard.

For most researchers, however, the more pressing problems in evaluation research are methodological. How much credence can one give to student testimonials or to the teacher's estimate of the success of a course? How can one isolate the variable he wishes to measure, and so be sure that changes he notices are not due to extraneous factors? What should be the criterion

¹Based on a paper presented at the 1959 meetings of the National Council on Family Relations.

of success in a family life course? When should the measurement be made: at the end of the course, six months later, or sixty years later? And, of course, there are a variety of questions concerning what kinds of instruments to use, what kinds of statistical analyses are proper, and other problems generic to all research (3, 4).

AN EVALUATION RESEARCH PROGRAM

As an educational institution which is designed to be flexible and experimental in its teaching method and content, and which has traditionally emphasized a sensitivity to the results of its education on its students, The Merrill-Palmer Institute is a logical place for a continuing program of evaluation research. In the past, this program has included studies of the effects of a short-term program for dietitians, the outcomes of summer workshops, the learnings that take place at summer camp, and the results of teaching preparation for marriage by two different methods. Each year "reactionnaires" are collected from both undergraduate and graduate students to gain clues about the success or failure of the academic programs.

Several recent studies have attempted to measure specific changes in beliefs and attitudes which took place in female undergraduate students during their study of human development and family life at the Institute. The studies have usually utilized schedules and specially constructed Likert-type scales administered to experimental groups composed of these undergraduates and to control groups in nearby universities both at the beginning and the end of the test period. Technical reports describing in detail the methods used in collecting and analyzing the data are available from the author upon request. In the present paper the results of these studies will be summarized and discussed briefly.

1. Attempts were made to measure the degree to which students are "child-centered" in their philosophies of family living. In the first test, the experimental group changed very little during the quarter in its degree of child-centeredness, but this small change in the direction of less child-centeredness when coupled with a control group change of similar magnitude but in the opposite direction created a significant difference between the two groups. When the scales were refined and rerun on two new samples, no statistically significant differences were found, although this time the experimental group did become slightly more child-centered than the control group.

2. An experimental group became significantly more self-confident about child-rearing abilities during the quarter, as did a control group. With improved scales, a similar test showed the experimental group becoming more confident and the control group less confident, but the changes were not statistically significant.

3. As the quarter passed, girls in the experimental group became more accelerating in their concepts of child development (that is, they expected

children to be able to perform specific behaviors at younger ages than they had previously expected); control group girls became less accelerating during the same period.

4. A sentence completion test showed that neither an experimental group nor a control group changed much during a semester as far as degree of positiveness toward family life was concerned, but that there was a statistically significant difference between the groups at both the beginning and the end of the test period, with the experimental group of students being more positive toward family life in both instances.

5. Responses to a variety of individual items (not a scale) indicated that both experimental and control group students increased their belief that parents should reduce their dependency on "experts" in child rearing, and that the experimental group had less reliance on the experts than did the control group; that marriage and parenthood were the highest goals of life for fewer experimental group girls at the end of the quarter than at the beginning, and for fewer experimental group girls than control group girls at any time; that more experimental group girls believed they had good self-understanding at the end of the quarter than at the beginning, but that all the control group girls believed they had good self-understanding both at the beginning and at the end of the test period.

6. On two different occasions a special test was run to see how the students would estimate changes that had taken place in their own beliefs and attitudes and to compare these estimates with what the scales showed these changes actually to be. Both individual changes and group changes were compared with student estimates. Students were found to be poor estimators of the direction the class as a whole had moved on various dimensions measured by the scales, guessing the wrong direction about as often as the right one. They were even less accurate in estimating the direction of change in their own personal attitudes and beliefs. These findings were obtained for both experimental and control group students.

7. Of greater importance than the above point, it was found in two different test runs that instructors also experienced difficulty in estimating the direction of change in their own classes (using the students' actual scale scores as the "true" measurement, of course), although they were better able to make such estimates than were the students themselves. Various instructors in the experimental and control groups predicted changes in individual students correctly from one-fifth to one-half of the time, and were right about the direction of change for the class, as a whole, from two-thirds to four-fifths of the time. Although it is difficult to generalize about the direction of the errors made in the instructors' estimates, it is true that there was a small trend toward the instructors' overestimating the degree to which students changed in socially approved directions (more confident, more positive, etc.). Perhaps a more important generalization is that a greater relationship existed between the estimates of the instruc-

tors and the estimates of the students than between either of these estimates and the test scores themselves. Instructors and students, it seems, shared a common estimate of what was happening to the students even when it apparently was not happening at all.

8. Research now in process includes another analysis of changes in the expectations of students regarding the ability of children to perform specific acts at various ages and a study of changes in the student's ability to perceive and conceptualize academic problems.

SOME CONCLUDING REMARKS

Certain problems have arisen during the course of the researches noted above. The question of adequate controls in measurement of change has always been a difficult one in evaluation research, but it has been particularly bothersome in these studies since the control groups had to be drawn from institutions other than that which had the experimental groups. This led to differences existing between the two groups at the beginning of the test period on the very dimensions which were being measured. These differences themselves could have been controlled if large enough samples could have been drawn; however, due to the nature of the studies, only small samples of students were available. The use of small samples, again, raised problems of adequate N's for some kinds of analyses and decreased the likelihood of statistically significant results emerging. Having differences exist between experimental and control groups at the beginning of the test period made analysis more difficult, but it did provide leads to another question educational institutions ask: How are our students different from other students *when they come to us*? Compared to control group students, these experimental group students could be characterized at the beginning of this educational experience as rather family-centered people, though not particularly child-centered, who have need of support for their future child-rearing practices and understandings (at the time of the studies none of the subjects had children).

Within the limitations of sample size, scale sensitivities, and control group problems, however, the picture that emerges is that after a relatively brief educational experience the students in the experimental group have increased confidence in their ability to rear children and increased expectations concerning the behavior of children. They also tend to decrease their reliance on expert help in child rearing as their self-confidence increases.

Possibly of most interest to evaluation researchers generally were the findings regarding the low ability of students to judge their own changes in beliefs and attitudes. The idea that an individual is the best judge of his own learnings finds little support in these data. Also, the students proved to be relatively poor judges of the changes which took place in the class as a whole.

Instructors, it appeared, were better than students in judging changes that took place in the classes but not so accurate as is usually assumed in the educational world. Merely asking instructors what took place in their courses would be an inadequate way of determining what actually did take place. All of this, of course, depends on the comparison made between student or instructor estimates and the scale scores; and that, in turn depends on the accuracy and adequacy of the scales. The fact that changes in test scores were often small and below the level of statistical significance further makes generalization difficult, as does the fact that two testings of the same dimension did not always produce similar results.

The ongoing program of evaluation research has demonstrated several other things. It has become obvious that large time, staff, and money expenditures are necessary. Hopefully, it has become equally obvious that although such research is costly and difficult it deserves to be an integral part of any educational program. What is still not at all obvious, however, is whether the results of such research will prove to be of practical use in the improvement of the teaching process. It remains for the researcher to find ways of making his studies more meaningful and important and of interpreting his results so as to make them usable by the teacher.

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MARITAL ROLES OF THE RETIRED AGED¹

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In searching for an understanding of the factors that have rendered the position of the aged increasingly difficult in the United States, it is helpful to view the age roles in relation to the kinship and occupational systems; by juxtaposing these systems and age roles we can locate some of the cultural contradictions and discontinuities that help create strain.

In an urban industrialized society such as ours, some of the most emphasized values are work, productivity, and achievement. The cultural source of these and related values is generally assumed to be the Protestant Ethic. Our socialization process understandably reflects these values, and to attain them one is inculcated with the ideals of competition, punctuality, diligence, speed, orderliness, and work, in the hope that the personality system will be motivated to fulfill specific structured adult roles in the occupational social system. The male learns, for example, that "in our society economic success is the very touchstone of manliness" (1, p. 162).

The internalization of these values is functional for the occupational social system as well as the individual personality system, since both systems interact in a society approaching an achievement orientation (with emphasis on performance, as contrasted to an emphasis on attributes).

This occupational system is also interrelated with the family system, which has been structured in response to the requirements of our industrial occupational system. In fact, with the absence of strong extended kin relations, our conjugal family is well adapted to the exigencies of the occupational system, with its mobility requirement. The conjugal type family is probably the least exposed to strain and possible disruption by the dispersion and mobility of its members (a frequent phenomenon in an urban industrialized society). In the United States, the conjugal family is articulated into the economic productive structure through the male's occupational role, and it is usually the husband who is the point of juncture between these two social systems.

Not only does the socialization process of the family transmit and inculcate the achievement and work value system, creating internalization in the personality system, but these values are further reinforced by being

¹Revised version of paper read at the 54th annual meeting of the American Sociological Society, September, 1959. This research has been supported by the Forbes Hawks Gerontological Fund and by a National Institute of Health grant, HTS-5208(C2), to the Geriatric Clinic of the University of Miami.

institutionalized in the family system. The male's occupational status, for example, is one of the most fundamental statuses in both the family and society. Parsons goes so far as to say: "... a mature woman can love, sexually, only a man who takes his full place in the masculine world, above all its occupational aspect, and who takes responsibility for a family" (2, p. 22). Legal negative sanctions enforce this family role expectation for the male, since in every jurisdiction except Mississippi it is a criminal or quasi-criminal offense for a husband to fail to provide economic support for his wife (5).

This interrelation and interpenetration of the family and occupational systems is further reflected in the role expectations that women have for husbands. In a nationwide survey of the American Institute of Public Opinion, a representative sample of married men and women were asked what they considered to be the most important quality in a good husband and wife. The most frequent answer given by the men for women was "good homemaker," "good housekeeper," etc. This corresponds to Parsons' conception of the woman's fundamental status as her husband's wife, who is responsible for the internal affairs of the family, such as management of household activities, care of the children, etc. The most frequent answer women gave for the most important quality of a good husband demonstrated a somewhat different conception of the traditional male role: a cooperative partner. Almost half of the women (42%), however, mentioned "being a good provider" as an important quality of a husband (4).

A study of 100 retired married couples over sixty years of age residing in the Greater Miami area (Dade County, Florida) was undertaken to discover the role conception of the retired male and female and whether husband and wife agreed on their role conceptions.

The sample, selected from almost all of the census tract areas of the county, had the following salient characteristics: Over 80% of the couples were migrants from other states, predominantly northern and northeastern, the median number of years they had resided in the Miami area being seven; the median and mean age of the wives was 64 years, and that of the husbands, 69; the median number of years married was 39½; the group was heavily represented in the upper educational and occupational levels.²

The following question, a modification of the one used in the Public Opinion survey, was asked of both husband and wife, independently of one another: "What would you say is the most important quality of a good husband (wife) who is past sixty years of age?"

²Because the Dade County population has changed radically since 1950, and because the special census of 1955 merely tabulated age, sex, and race categories, adequate data on the number and characteristics of the aged are unavailable. There is therefore no way of determining how the characteristics of this sample deviate from the total aged population in Dade County. Other studies of Florida migrants indicate that upper economic groups were heavily represented.

For the female role expectation, three main response categories emerged: (a) qualities of love and understanding, (b) companionship and compatibility, and (c) good housewife, etc. Of these three most frequently mentioned categories, the most important role for the woman was an expressive one: 38.1% of the male responses and 38.2% of the female responses mentioned such personality characteristics as understanding, consideration, devotion, loving, etc. The next most frequent characteristic mentioned by both male and female was companionship: 28.2% of the male responses and 21.6% of the female responses mentioned it. Only 15.4% of the male responses and 15.7% of the female responses indicated that any kind of instrumental work activity such as good housewife, cook, etc., was an important quality in a good wife past sixty. There was no significant statistical difference between the opinions of the husbands and wives with regard to the most important quality of a good wife past sixty.

In the qualifications of a good husband, four main response categories emerged: (a) qualities of love, understanding; (b) companionship and compatibility; (c) helps wife; and (d) good economic provider. Utilizing these four most frequently mentioned categories, we discovered that the women emphasized expressive or emotional qualities for the men much more than the males did for themselves: 47% of the female responses claimed that the qualities of considerateness, understanding, patience, etc., were important for a good husband past sixty years of age, whereas only 30.6% of the male responses mentioned these qualities. There was marked agreement on the traits of companionship and compatibility: 22.2% of the male responses and 22.5% of the female responses indicated that these were considered important for a good husband. The most striking difference between male and female, however, was that *the men appeared reluctant to relinquish any productive meaningful work activities in the marriage relationship, whereas the women seemed more willing to allow them to relinquish these activities*. While 11.1% of the male responses claimed that being a good economic provider was the most important quality of a good husband past sixty years of age, only 2.9% of the females mentioned this trait. Again, in regard to work activities, whereas 14.8% of the male responses mentioned helping one's wife as an important trait, only 4.9% of the female responses mentioned it. There is a highly significant statistical difference between the opinions of husbands and wives with regard to the most important quality of a good husband. The chi-square test indicates that these differences reach the .01 level of significance.

From these figures, it would appear that some of the males are still clinging to an instrumental role conception in their family system, even when they are retired from active participation in the occupational system. Some, who mentioned being a good provider, continue to think of their link to the occupational system; others attempt a substitute instrumental activity: helping their wives. One of the factors accounting for the strain

of the retired male would appear to be not so much a function of the disagreement of the husband and wife as to the male role, but this unrealistic male adherence to his preaging role pattern that emphasized work activity.³

Internalizing the value system of a society that approaches an achievement orientation seems to be functional for the personality, family, and occupational systems *when these achievement goals of action are attainable*; it may create strain, however, when on retirement the individual is prevented from attaining these internalized ends of action.

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³Some indication of the strain present in the role of the aged retired male can be found in the results reported by Powell (3). Retired males over sixty-five years of age had a suicide rate almost twice that for all males of the same age.

Book Reviews

GROWTH DIAGNOSIS: SELECTED METHODS FOR INTERPRETING AND PREDICTING PHYSICAL DEVELOPMENT FROM ONE YEAR TO MATURITY. Leona M. Bayer and Nancy Bayley. xiv+241 pages. The University of Chicago Press, Chicago, 1959. \$10.00.

This book was prepared for "physicians who deal with children and adolescents." The authors' main purpose is that of providing "a set of usable blueprints" to aid medical clinicians in "ruling out or identifying the presence of significant growth deviation."

Growth diagnosis is presented as playing an antecedent role in (a) arriving at certain clinical diagnoses and (b) evaluating some prescribed treatments. In the first role, it is concerned with initially describing a child's morphologic status (e.g., "tall") and progress (e.g., "fast-growing"). The physician, supplied with such information, seeks to determine the extent to which these conditions are "constitutional," associated with "pathological pituitary activity," and so forth. In the event medical intervention is undertaken, growth diagnosis performs its second role by depicting morphologic rates of change which "provide checks on the progress of treatment."

Emphasis throughout the book is "on practical application, not on describing the research behind it." Part I presents a number of methods for collecting, recording, and evaluating data pertaining to morphologic status and progress. Part II exhibits "specific applications" of these methods on 22 individuals chosen to exemplify the wide assortment of "patients with developmental problems." Quantitatively, the content of the book is 60 per cent case illustrations (Part II), 25 per cent methodology (Part I), and 15 per cent prefatory text, appendixes, and bibliography.

It is recommended that the physician make use of "qualitative impressions" from inspection of the child; direct measures of stature, stem length, shoulder width, hip width, and body weight; roentgenograms of the left hand and wrist; and photographs of the nude body from the front, rear, and left side. He is given instructions on obtaining these materials and on interpreting them through a series of deviation indicators. The series includes: charts adapted from Gray and Ayres (1931) to yield a ten-item profile for body size and form at successive annual ages; graphs (involving the combined use of stature, hip width, and body weight) constructed by Bayer and Gray (1935) for characterizing body form and appraising the satisfactoriness of body weight at successive ages; a fourteen-item rating profile developed by Bayley and Bayer (1946) for assessing sexual differentiation in body form; charts adapted from Nicholson and Hanley (1953) for appraising the development of the penis and testes in boys, the breasts in girls, and the pubic hair in both sexes; the Greulich and Pyle (1959) standards for ossification in the hand and wrist, with the interpretation that a difference between skeletal age and chronological age "of three years or more is considered very deviant";

graphs developed by Bayley (1956) to typify stature and weight, age by age, of children at five spaced points in corresponding distributions for ossification in the hand and wrist; tables for predicting early adult stature (based on sex, age, attained stature, and ossification in the hand and wrist) constructed by Bayley and Pinneau (1952); and growth-rate graphs for "evaluation of growth during brief periods, especially for comparing treatment periods with control periods."

The authors and publishers have made exceptionally generous use of charts, photographs, and tables. Of the 145 pages comprising Part II, the composition is less than one-fourth text and more than three-fourths graphic, pictorial, and tabular material. In this reviewer's opinion, many of the illustrations in Part II are unnecessarily space-consuming. By contrast, the explanatory text sometimes appears sparse with respect to identification of the pertinent steps in an individualized growth diagnosis and explanation of their helpfulness in reaching the clinical diagnosis.

Those abreast of research on the physical development of children will wish that *Growth Diagnosis* were somewhat broader in scope. They will miss deviation indicators for muscle and fat components of body mass, calcification and eruption of teeth, and/or selected physiological and biochemical variables. Some, recalling that distributions for body weight at childhood and adolescent ages are skewed, will consider this grounds for dissatisfaction with the evaluation of body weight in terms of standard deviation units.

The book includes several case-study observations on the presence and absence of associations among human traits. Three examples follow. Child A's "medical history includes mumps at 2 years 6 months and a series of acute colds, with bronchitis between the ages of 5 and 6 years. There is, however, no evidence that these illnesses had any lasting effects on the course of growth." Child X, an exceptionally short seven-year-old boy with undescended testicles was prescribed oral androgen "intermittently for five years; the testicles descended with each administration of steroids, receded with withdrawal of medication." Clinical study of two exceptionally tall youths, one from "a tall family" and the other from "a family of average height," yield "evidence neither of pituitary hyperactivity nor of gonadal deficiency."

Bayer and Bayley have developed their book on the conviction that "repeated morphological observations on growing patients can contribute certain insights into the total understanding of the growth process which laboratory and clinical studies alone cannot provide." To the extent that the book guides physicians toward a practicable appreciation of the soundness of this conviction, it will perform a helpful service.

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NATURAL CHILD REARING. Eve Jones. 278 pages. Free Press, Glencoe, Illinois, 1959. \$4.95.

This book is described by the publishers as a practical manual for reasonable parents. The author in the foreword suggests that the book be read through to get the basic philosophy that psychoanalytic psychology offers about child care and development instead of using it as a handbook. Many readers, however, will be

tempted to glance through the book, reading the section headings, paragraph headings, and the statements in italics instead of reading the book word for word. The detailed index also invites the reader to look up a topic of special interest.

The author's orientation in psychoanalytic psychology is evident throughout the book. Parents lacking an understanding of this point of view in psychology will be able to accept some chapters more readily than others. The discussion of care during infancy will be easier to accept than the chapter on sexuality.

Although Dr. Jones covers all age levels from infancy through adolescence, she devotes more space to discussing care during infancy, particularly the period immediately after birth. The period of middle childhood receives very little attention, and 40 of the 278 pages are used for a discussion of adolescence.

As compared with other recent books written for parents about children, this book is more specific in stating what to do and what not to do. Although the reasons behind the directives are stated quite clearly, the book implies to the reviewer that there is only one appropriate method of handling many situations. Some parents will welcome this specificity of directions, but the value of this approach seems questionable in the light of what is known about parent education.

The reviewer found the discussion of the nursery school inadequate and in some ways misleading. The author states that a child should be enrolled in nursery school as soon as he is eligible, if a good nursery school is available. Considering that the author includes the discussion of the nursery school in the same chapter that she discusses the two-year-old, and considering the wide variation in age when children are ready for nursery school, this is a misleading suggestion to parents who have no understanding of nursery education. The description of a good nursery school is inadequate, and the reader is not referred to any other source for this information. For example, the statement is made that the nursery school teacher should be informed, but no information is given about the extent of formal training needed to be an informed nursery school teacher.

To the parent having difficulty with setting limits, the chapter on accepting responsibilities should be helpful. In the chapter on adolescence, the author presents an interesting theory about guidance in relation to sex in early, middle, and late adolescence. The regularity with which adolescents pass through the described stages in gaining control of sexual impulses could be questioned.

This is a highly readable book, and the author carefully defines the psychological terms. Parents conversant with psychoanalytic psychology and wanting help in applying this theory to child rearing will find at least parts of this book helpful. Parents knowing nothing of psychoanalytic psychology or unwilling to accept this theory will find other books on child rearing more useful.

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Book briefs

THE PATIENT AND THE MENTAL HOSPITAL: CONTRIBUTIONS OF RESEARCH IN THE SCIENCE OF SOCIAL BEHAVIOR. Edited by: Milton Greenblatt, Daniel J. Levenson, Richard H. Williams. 658 pages. The Free Press, Glencoe, Ill., 1957. \$6.00.

This is the kind of book that cannot be reviewed, but must be reported. Feeling that it is time to take a careful look at mental hospitals from the outside, rather than letting developments come about as the result of political and other pressures, a group of more than 50 social scientists have focused their attention upon the patient and the mental hospital. One of the underlying convictions throughout the book is that the recovery of the mental hospital patient depends not merely upon specific treatment procedures, but, perhaps even more, on the sociopsychological characteristics of the hospital community. As various specialties work within this community, it becomes not only a valuable "living laboratory" for the practical application of existing knowledge, but also a research domain where new theories and techniques can be developed and tested.

PART ONE deals with mental hospital organization and its implication for treatment. PART TWO is devoted to a study of therapeutic personnel, and PART THREE to the hospital ward. PART FOUR treats the patient and the extra-hospital world, including out-patient care and community participation through volunteer movements. PART FIVE pulls together the implications of these studies for psychiatry and hospital practice and for new theoretical formulations. The book ends with a critical appraisal of the mental hospital as a research setting.

The critical student of mental illness and the many problems of treatment both in an institution and on a community basis will find this an invaluable resource.

BEFORE THE CHILD READS. James L. Hymes, Jr. 96 pages. Row, Peterson and Co., Evanston, Ill., 1958. \$2.00.

Written in Hymes' inimitable chatty style, this book is addressed to the feminine primary school teacher but is of value to any persons—parents included—interested in the education of young children.

Hymes inveighs against attempts to *build* readiness into children at the prereading stage by means of drill and structured exercises; instead he encourages capitalizing upon the existent readiness to discover the how and why of community activities and to enjoy the written word through listening to stories. The author aptly points out that though children have not started reading they have been using words and thinking for a long time, and are more likely to be bored than motivated by sterile simplicity in verbal matter.

Hymes advocates the observant thoughtful teacher who knows her pupils as individuals, who uses her charm and allurements to garner materials and experiences of meaningful interest to young children, who gets to know parents as friends to the extent of being on a first-name basis.

The author states that readiness "will not blow away" but "grows deeper and stronger . . . with time and good living."

WHAT PSYCHOLOGY SAYS ABOUT RELIGION. Wayne E. Oates. Association Press, New York, 1958. \$.50.

This small paper back volume is not a reprint but is written for the lay reader interested in the ways psychologists view religion. It offers a readable and compact discussion of psychological interpretations of religion in terms of "bondage to idols," "childishness," "a sickness," "an illusion" or "the search for ultimate meaning in life." What contemporary psychology has to say about religion is divided into four stages: "the silent treatment," "a blatant noisy rejection," "a critical but cautiously affectionate attitude" and "an unhampered voice of conviction." The closing chapter suggests directions in which psychology needs to go "beyond itself" to explore yet "uncharted realms of human nature." An annotated bibliography and a helpful set of notes add to the usefulness of this book.

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